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ABSTRACT

In 1981, Chile introduced nationwide school choice by providing vouchers to any student wishing to attend private school. As a result, more than 1,000 private schools entered the market, and the private enrollment rate increased by 20 percentage points, with greater impacts in larger, more urban, and wealthier communities. Using differences across roughly 300 municipalities, this paper shows that the first-order effect of this program was increased sorting, as the "best" public-school students switched to the private sector. A simple method was used to make the more general point that if choice leads to sorting, then one cannot determine its impact on achievement solely by assessing whether public schools improve in response to competition or by measuring whether students benefit from attending private schools. Rather, one has to look at changes in aggregate outcomes in entire educational markets. Finally, using test scores, repetition rates, and grade for age as measures of achievement, no evidence was found that the large reallocation of students from public to private schools improved average educational performance in Chile. (Contains 48 references, 12 tables, and 12 figures.) (Author/RT)

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National Center for the Study of Privatization in Education

Teachers College, Columbia University

When schools compete, how do they compete?
An assessment of Chile's nationwide school voucher program

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Abstract—In 1981, Chile introduced nationwide school choice by providing vouchers to any student wishing to attend private school. As a result, more than 1,000 private schools entered the market, and the private enrollment rate increased by 20 percentage points, with greater impacts in larger, more urban, and wealthier communities. Using differences across roughly 300 municipalities, we show that the first-order effect of this program was increased sorting, as the "best" public school students switched to the private sector. We use a simple model to make the more general point that *if* choice leads to sorting, then one cannot determine its impact on achievement solely by assessing whether public schools improve in response to competition, or by measuring whether students benefit from attending private schools. Rather, one has to look at changes in *aggregate* outcomes in entire educational markets. Finally, using test scores, repetition rates, and grade for age as measures of achievement, we find no evidence that the large reallocation of students from public to private schools improved average educational performance in Chile.

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1 Introduction

The notion that free choice is welfare-enhancing is one of the foundations of modern, market-oriented societies. This view is prominent in the school choice debate, where there is a widespread perception that public schools are inefficient local monopolies, and that the quality of education would improve dramatically if *only* parents were allowed to freely choose between schools. For example, in recent work Hoxby (2001) asks "what is the range of productivity over which choice could cause productivity to vary? Recent history suggests that school productivity could be much higher than it is now - 60 to 70 percent higher."

Two arguments underlie the view that choice would improve the quality of education. First, there is a widely-held belief that private schools are better than public schools. Although most research on this is hampered by selection issues, recent work that exploits quasi-experiments with vouchers find *some* evidence that children benefit from attending private schools.[1] The implication is that unrestricted choice could raise students' achievement merely by facilitating their transfer to the private sector. A second, perhaps even more compelling argument for choice comes from our instinct that people and organizations respond to incentives. Therefore, by correctly aligning the incentives public schools face, choice would force their ossified bureaucracies to improve.

The paper assesses these arguments by examining the impact of a nationwide school voucher program introduced in Chile in 1981. Three reasons account for why this experience provides an excellent opportunity to understand the consequences of school choice. First, Chile's government established a "textbook" voucher scheme, by providing vouchers to any student wishing to attend private school, and by directly tying the budgets of public schools to their enrollment. Second, this program, whose essential features remain unchanged 20 years later, created a dynamic educational market: more than a thousand (often for-profit) private schools entered the market, and the private enrollment rate increased from 20 to 40 percent by 1988, surpassing the 50 percent mark in many urban areas. Third, Chile has excellent micro data, including ongoing nationwide tests introduced at the beginning of the

[1] See Rouse (1998), Howell and Peterson (2000), Peterson, Myers and Howell (1998), Wolf, Howell and Peterson (2000), and Angrist, Bettinger, Bloom, King and Kremer (2001) for analyses of experiments in Milwaukee, Dayton, New York, Washington D.C., and Colombia, respectively.

reform. We use such information to measure the impact of the competitive forces unleashed by the voucher program.

To preview our bottom line, we find that although school choice led to dramatic changes in the school market, there is no evidence that it improved average educational outcomes. As one piece of suggestive evidence, consider the results from international tests in science and mathematics, widely known as the TIMSS, in which Chile participated in 1970 and 1999. Using these exams, we can assess whether 20 years of unrestricted school choice have improved Chile's performance relative to the other 12 countries that also took part in both years. The evidence, presented in figure 1, shows that its relative ranking, if anything, has worsened. This is all the more surprising because Chile's economy has generally done quite well during this period, growing faster than that of the other countries featured.[2]

This presents an enormous puzzle. How can we reconcile this result with the perception that, in Chile as well as in the U.S., private schools are better than public schools? More importantly, how do we square it with our instinct that when parents are able to choose between schools, they will select the most effective one available, and that schools should respond by improving their performance?

This paper suggests that the answer to the puzzle is not that parents do not select the best schools for their children, or that schools do not respond to incentives, but rather that the way in which schools strive to improve, and the manner in which parents evaluate educational quality, may not be as simple as school choice advocates assume. This is because the education industry is unique in two respects. First, one of the most important determinants of a school's quality is the underlying quality of its "customers". Second, parents hold a strong belief that their children will benefit from interacting with better peers. This has two implications for thinking about the potential effects of choice. First, if a school is given incentives to improve, it is at least as likely that it will attempt to do so by attracting better students, as by raising its productivity or value added. Second, when parents are free to choose between schools, they will tend to select those that provide "good" peer groups for their children. Because of these complementary forces, choice will tend to result in more

[2] From 1970 to 1999, per capita GDP grew at an annual rate of 4.3 percent in Chile, and 2.8 percent in the other 12 countries (authors' calculations using the International Financial Statistics).

stratification between schools.

A central argument of this paper is that such sorting is critical in thinking about how to assess the effects of comprehensive school choice. Specifically, we build a model that shows that if choice results in greater segregation, then one cannot determine its impact on achievement solely by assessing whether public schools improve in response to competition, or by measuring whether students benefit from attending private schools. Rather, one has to look at changes in aggregate outcomes in entire educational markets.

To see this, note that even if competition does force public schools to improve, their average achievement may fall if their best students move to the private sector. Because of this "cream-skimming", it will be nearly impossible to isolate the effect of choice on public schools' productivity from that on the composition of their student bodies. Additionally, if choice leads to sorting, then even experimental studies may not reveal whether a large-scale voucher program would raise achievement by shifting students to the private sector. Intuitively, a randomly picked student might benefit from attending private school, but as long as part of this benefit is due to a better peer group, it is not clear that it would persist if one transferred a large number of students, since this would cause the quality of private school peer groups to decline. Finally, the model illustrates that the best one can do is to approximate a weighted average of these two effects (the private school advantage and the effect of competition on public performance) by measuring the change in the achievement of *all* students in a given market.

In short, sorting greatly complicates the analysis of the effect of school choice on educational productivity. Yet even though it is widely acknowledged that it might be a consequence of school choice,[3] the existing empirical literature largely ignores this issue.[4] Clearly, a reason for this is that much of the available evidence on choice comes from small-scale experiments with school vouchers, and one simply cannot study sorting in this context because there are

[3] Sorting is central in the theoretical models of vouchers by Manski (1992) and Epple and Romano (1998). It also plays a critical role in the theoretical literature on inequality and education, see for example Benabou (1996a,b) and Fernandez and Rogerson (1996, 1998).

[4] An exception is the recent work by Berry, Jacob, and Levitt (2000) on an open enrollment program in Chicago. Although it does not use pre-program information, this paper provides suggestive evidence that open enrollment led to increased segregation between public high schools. Fiske and Ladd (2000) provide similar evidence for a comparable scheme in New Zealand.

few children switching between schools or sectors.[5] Small scale experiments also hamper the study of productivity effects, since only a small part of the market is effectively in play, which blunts competitive pressures. In addition, doubts surrounding the eventual reach and duration of these experiments discourage market entry by new private schools, and may prompt many parents to adopt a "wait and see" attitude.[6]

Which brings us back to the Chilean case, as it is the only experience we have so far with comprehensive, sustained school choice. As discussed, the program had a large effect on the nationwide private enrollment rate, but we show that it had a much greater impact in larger, more densely populated, and wealthier communities. We measure the program's impact by comparing schools and individuals in areas where it led to large increases in private enrollment, with markets where it had a smaller effect, using differences across roughly 300 municipalities. We use three types of information to assess these effects: 1) school-level data from nationwide tests introduced at the beginning of the reform; 2) administrative data on enrollment and repetition rates for every school in Chile since 1980; and 3) micro data from the 1982 population census and successive rounds of the national household survey.

Our results suggest that the first-order consequence of vouchers in Chile was a massive exodus from public schools by families from higher socioeconomic backgrounds. Specifically, we show that municipalities with large increases in private participation display: 1) significant declines in the relative socioeconomic status of students in public schools; 2) large declines in public school average test scores (relative to the municipal average); and 3) increases in the relative repetition rates in public schools. In addition, we demonstrate that if one does not take sorting into account, then one could erroneously conclude that private schools in Chile are better than public schools, and that public schools do worse in regions where the private enrollment share is larger.

Further, once we correct for the direct effect of sorting by comparing *aggregate* changes across educational markets, we find no evidence that the introduction of school choice led to

[5] For example, Milwaukee's initiative, the oldest and largest in the U.S., was originally limited to one percent of public school membership. The number of students using vouchers in Colombia peaked at three percent of secondary enrollments in 1995.

[6] Most voucher programs in the U.S. eventually face legal challenges (as is currently happening to the Cincinnati initiative). Sustainability problems are also a feature in other countries: the Colombian program considered by Angrist et al. (2001) was discontinued in 1997.

productivity improvements. Specifically, we show that over the 1980's: 1) average repetition rates, 2) average test scores, and 3) average years of schooling among 10-15 year-olds, did not change any faster or slower in municipalities where the voucher program had a large effect, relative to municipalities with little change in private enrollment. Because these results are obtained by comparing communities where the marginal return to private schooling is likely to be higher with those where it is probably lower, we argue that our estimates are likely to overstate the impact of choice in a randomly selected community. In addition, we find evidence of pre-existing *upward* trends in educational outcomes and wealth in municipalities with large increases in private enrollment, and thus argue that even the average *marginal* impact of choice on educational productivity is lower than what our estimates imply. We thus conclude that the main effect of school choice in Chile has been to facilitate the exit of middle class families from the public school system, without improving average educational outcomes.

The paper proceeds as follows. We begin by reviewing the institutional details of Chile's voucher program, and then describe its impact on the school market. We proceed to sketch a simple model of vouchers that allows for both sorting and productivity effects, and discuss how the usual empirical approaches fit into this framework. Moving on to results, we show that sorting was a key consequence of school choice in Chile, and that it has a significant effect on the analysis of educational quality. The final results focus on how choice affected achievement, and also check for possible biases due to pre-existing trends.

2 Chile's school voucher program: A brief overview

In 1981, as part of the Pinochet government's sweeping market-oriented reforms, Chile introduced a nationwide school voucher program. The easiest way to understand the nature of this reform is to discuss how it modified the manner in which schools were governed and funded. To begin, before the reforms, there were three types of schools in Chile.

1) *Fiscal schools.* These public schools were controlled by the national Ministry of Education, which was responsible for all aspects of their operation. It hired and paid teachers, maintained facilities, and designed the curriculum. In 1981, 80 percent of all students

were in such institutions.

2) *Unsubsidized private schools.* These private institutions did not receive public funding. They charged relatively high tuition and catered primarily to upper income households. Prior to the reforms, they accounted for about 6-7 percent of enrollment.

3) *Subsidized private schools.* These institutions did *not* charge tuition, received public subsidies, and were generally religious.[7] The size of the subsidy they received depended on the government's fiscal condition, but averaged about 50 percent of nominal per-student spending in the fiscal schools. This aid was supposed to be disbursed at the end of the school year, but was typically delayed by several months, resulting in its erosion by inflation.[8] Prior to the reform, these schools accounted for 15 percent of enrollment.

The 1981 reforms effectively created a nationwide voucher program with financial incentives for both public and private institutions. This initiative had three main components:

1) *Decentralization of public schools.* Fiscal schools were transferred from the Ministry of Education to roughly 300 municipalities or communes, such that they became known as *municipal* schools. The contract between the Ministry and the national teachers' union was abrogated, and public school teachers had to either transfer to municipal schools as common public employees, or resign and reapply for teaching jobs as regular private sector workers. To encourage the latter, the Ministry offered substantial severance payments.

2) *Public school funding.* Municipal schools continued to be funded centrally, but each municipality started to receive a *per-student* payment for every child attending its schools. As a result, enrollment losses came to have a direct financial effect on the municipal education budget.

3) *Public funding for private schools.* Most importantly, (non tuition-charging) subsidized private schools began to receive exactly the same per-student payment as the municipal

[7] Espínola (1993) states that in 1970, 53 percent of private schools were Catholic and the remaining were Protestant or run by private foundations. The predominantly religious nature of these institutions in part reflects that they required outside funding, which was generally more available to religious schools.

[8] See Schiefelbein (1971). Inflation averaged 5.2 percent per month in the 1970s. Assuming that public school teachers are paid every month, this means that the real value of the stipend would be only 35 percent of real per-student expenditures in the public sector if the stipends were paid on time (at the end of the school year), and 26 percent if the payments were delayed by 6 months.

schools.[9] These payments were distributed on a monthly basis, and their initial level was set 30 percent higher than the pre-1981 average spending per student in the public sector.[10] To distinguish these institutions from the subsidized private schools that existed before the reforms, we will call them *voucher* private schools.[11]

Tuition-charging private schools mostly continued to operate without public funding. While they could have stopped charging tuition and started to accept vouchers, these elite institutions in general chose not to participate in the voucher program. These schools were nevertheless also made part of a nationwide testing system that assessed 4^{th} and 8^{th} grade students every other year.

Finally, because voucher programs are often short-lived, it is worth mentioning that the essential features of this system have remained in place over the last 20 years. The center-left coalitions in power since 1990, have chosen to focus their efforts on channeling additional resources to "vulnerable" schools, increasing real educational spending and teacher salaries, and financially rewarding schools with high test scores (relative to given reference groups).[12] Nevertheless, they have left the core of the system – the per-student voucher payments and the freedom to attend any school, religious or not – largely intact.

3 The industrial organization effects of school choice

These reforms led to significant changes in the Chilean educational market. Figure 2 shows that the public sector's enrollment share hovered around 80 percent throughout the 1970's, but fell rapidly after 1981, dipping below the 60 percent level by 1990. The figure also describes the evolution of private schools' participation, which beginning in 1981, can

[9] The size of the voucher payment each school receives varies according to: 1) the educational level at which it operates, 2) whether it offers special programs, and 3) its distance from urban centers. Importantly, a given private school receives the same payment as a municipal school with similar characteristics.

[10] Matte and Sancho (1992).

[11] In Chile, they continue to be known as subsidized private schools.

[12] These are mainly policies aimed at: i) the worst performing schools – the P900 (*Programa de las 900 Escuelas*) program, ii) the entire K-12 system - the MECE (*Programa de Mejoramiento de la Calidad y Equidad de la Educación Preescolar y Básica*) initiative, iii) rural schools – the MECE-Rural, and iv) rewarding teachers in schools that perform well – the SNED (*Sistema Nacional de Evaluación del Desempeño de los Establecimientos Educativos Subvencionados*). Here we focus on the 1980's because it is the period in which the voucher program had its largest effects and was the key educational intervention, with the government refraining from compensatory initiatives.

be decomposed into that of voucher and tuition-charging schools. This makes clear that the rise of private enrollment in the 1980's is almost entirely due to the growth of voucher private schools. By 1986, only five years after the per-student payments were introduced, these schools' market share had doubled relative to that of the pre-1981 subsidized private sector, crossing the 30 percent level. In contrast, the market participation of the "elite" private schools remained roughly constant over the 1980's, and experienced a gradual but sustained increase during the 1990's.

The aggregate trends in figure 2 conceal considerable variation in the growth of the private sector across different educational markets. Using Chile's approximately 300 communes as proxies for such markets, figure 3 (panel A) presents kernel densities of the *change* in private enrollment ratios from 1982 to 1996. This figure considers all communes and also a subset composed of those with urbanization rates above 80 percent and populations above ten thousand, which we label urban communes. As one might expect, the voucher program had a larger impact in the latter group. By 1996, the median urban commune had experienced a 20 *percentage point* increase in the private share.

Table 1 presents additional evidence that vouchers had a larger effect in urban, populated, and wealthier communes. This table presents regressions of the 1982-88 change in the private enrollment share on several commune characteristics in 1982.[13] The results indicate that the growth in private enrollment after 1982 is strongly associated with population, urbanization, and the average years of schooling among household heads. For example, in a commune where the average parent is a high school graduate, private enrollment increased by 15 percentage points more than in a commune in which the average household head completed only primary school. Similarly, controlling for urbanization, the point estimates indicate that private enrollment grew by 5 percentage points more in a commune with 150 thousand people relative to one with a population of 50 thousand.

Over time, these differences have produced substantial cross sectional variation in private enrollment, as described in panel B of figure 3, which presents density estimates of private participation in 1996.[14] In roughly 40 percent of the urban communes the public sector has

[13] We postpone a description of the data used until section 4.

[14] As all other data presented henceforth, this figure refers only to the primary sector (grades 1-8).

become a minority player, and in extreme cases, it accounts for only 20 to 25 percent of all students. Further, this significant supply response was not limited to growth in pre-existing private schools. Figure 4 shows that more than 1,000 private schools were created from 1982 to 1985, increasing their number by almost 30 percent. In turn, while the incumbent private voucher schools that existed prior to 1982 were largely religious institutions, the new private voucher schools were often profit-seeking enterprises.[15]

A notable fact is that despite extensive entry in the private sector, and despite the sustained enrollment decline in the public sector, the aggregate number of municipal schools has barely fallen (see figure 4). Clearly, municipal officials have been unable or unwilling to close public schools. In spite of this, there has been a large reallocation of resources from the public to the private sector. First, because of voucher financing, the 20 percentage point enrollment shift shown in figure 2 means that a corresponding percentage of the Ministry of Education's school-related operational expenditures were reallocated to private schools. Second, this was accompanied by a large transfer of teachers, illustrated in figure 5. Although this reallocation was more gradual than the shift in enrollment, by 1990 the fraction of teachers working in the public sector had also fallen by 20 percentage points. This implies that despite significant changes in the relative sizes of public and private schools, the relation between the student-teacher ratios observed in the two sectors has essentially remained unchanged.

To summarize, the Chilean case provides a unique opportunity to analyze the effects of unrestricted school choice. To organize and frame our empirical analysis, the next section develops a model that while being general in nature, incorporates some of the institutional details specific to the Chilean experience.

4 A model of vouchers, sorting, and productivity

This section presents a model in which the provision of school vouchers has two effects. First, they may raise educational outcomes by shifting students to possibly more efficient

[15] Using a sample of urban communes for which we are able to construct a panel of schools (these communes account for about 70 percent of total enrollment in the country), we find that 84 percent of the new private schools in 1988 were private non-religious institutions.

private schools, and by forcing public schools to improve. Second, they can increase stratification by facilitating the exit of wealthier or more motivated households from the public system.[16] The central message of the model is that if choice leads to sorting, the proper way to measure the effect of choice on productivity is to consider its effect on aggregate achievement in entire educational markets.

4.1 Students and academic performance

To begin, consider a community in which students are indexed by i, distributed uniformly from 0 to 1, and assume i is arranged such that children with high socioeconomic status (SES) have high i's, and students with low SES have low i's. Formally, this is expressed in a simple parametrization of SES:

$$SES_i = \alpha \cdot i$$

so that SES ranges from 0 (for student i=0) to α (for student i=1).

We will assume that prior to the introduction of the voucher scheme, all students are enrolled in the public system. We also suppose that households care about academic performance, which for a given student is a function of her SES, and the average SES of the school she attends (a peer group effect). In the pre-reform scenario, student i's test score is:

$$T_i = \gamma SES_i + \lambda \overline{SES} + \phi \overline{SES} \cdot SES_i \qquad (1)$$

where γ and λ are positive constants. As long as $\lambda + \phi \cdot \alpha > 0$, which we will assume to be the case, every student benefits from going to a school with better peers, but the magnitude of this benefit can differ across students. If $\phi > 0$, students with higher SES benefit more from interacting with better peers; if $\phi < 0$, lower SES children derive the greater benefit.

[16] The contribution of this section is to provide a model of vouchers that has a simple closed-form solution, and to relate the implications of the model with the empirical work on school choice. The mechanisms in our model are closest to those in Manski (1992), who also allows for both productivity and sorting effects. Epple and Romano (1998) present a model of vouchers that also allows for sorting between private schools, as well as between the public and private sectors, but they do not allow for productivity improvements due to choice. They also allow private schools to provide financial aid to talented students.

In addition, we assume that all public schools are homogeneous, so that the average SES experienced by each student is simply that of the entire community ($\overline{SES}$).[17]

After the introduction of vouchers, private schools enter the market. If a student enrolls in the private sector, her test score is given by:

$$T_{i,priv} = \beta_{priv} + \gamma SES_i + \lambda \overline{SES}_{priv} + \phi \overline{SES}_{priv} \cdot SES_i$$

As this indicates, private and public schools have the same production function, except for the constant term β_{priv}. This term captures the productivity advantage of private schools relative to the public system (before the introduction of vouchers). In addition, note that the peer quality experienced by students who move to the private sector is no longer the average SES of the entire community ($\overline{SES}$), but rather that of students who switch to the private school ($\overline{SES}_{priv}$). In symmetry with the treatment of the public sector, we assume there is no sorting between private schools – all sorting occurs between sectors.

The voucher system may also prompt the public sector to improve. Therefore, we will assume that after its introduction, student i's test score in a public school is given by:

$$T_{i,pub} = \beta_{pub} + \gamma SES_i + \lambda \overline{SES}'_{pub} + \phi \overline{SES}'_{pub} \cdot SES_i$$

where β_{pub} measures the improvement in public productivity.[18] Note that the average SES of students in public schools ($\overline{SES}'_{pub}$) is not necessarily the same before and after vouchers.

4.2 Vouchers and Sorting

School choice critics often warn that vouchers may result in increased segregation as private schools "skim" the highest SES students from the public sector. One can think of several forces that might contribute to such an outcome. First, private schools may prefer higher SES children, and they might be able to exercise this preference if, unlike public

[17] Naturally, this will not be correct if there is sorting between public schools, or if there is sorting (e.g., through tracking) within each public school. It would be straightforward to allow for sorting within the public sector. Equation (1) also implicitly assumes that schools are nothing more than clubs, and do not produce any value-added. It would be easy to allow for a constant term reflecting some value-added by the public school, but this would simply be a renormalization of T_i, and would not affect our main conclusions.

[18] As will be clear later in the paper, we expect β_{pub} to depend on the degree of competition generated by vouchers.

schools, they are are allowed to reject students (as is the case in Chile). Second, higher SES parents might be more motivated and better informed, and therefore more successful at securing a higher SES peer group for their children. Third, it might be the case that high SES students benefit more from private schooling or, equivalently, that the cost of attending private schools is lower for them.

Any of these factors (or a combination of them) implies that private schools will "skim" the best students from public schools. Henceforth, we will *assume* that this in fact happens. In the appendix, we provide details on how two of these mechanisms fit into our framework.[19] To be clear, we do not view any of these mechanisms as definitive, but rather as simple illustrations of forces that could drive sorting. Ultimately, we will *test* whether the introduction of school choice in Chile led to greater segregation between public and private schools.

In our model, the key point is that sorting will affect the peer group each student experiences. Recall that before the voucher program, all students enroll in the homogeneous public system. The average SES of students in public schools, and the peer group experienced by every student in this sector, is therefore:

$$\overline{SES}_{pub} = \frac{o}{2}.$$

After the voucher program is implemented, higher SES students (say all students $i \in [b, 1]$) enroll in the private school and lower SES students ($i \in [0, b]$) remain in the public sector.[20] The average SES of public school students therefore falls to:

$$\overline{SES}^{v}_{pub} = \frac{ob}{2}.$$

The corollary is that the average SES of private school students is:

$$\overline{SES}_{priv} = \frac{o(1+b)}{2}$$

so that students who transfer to the private sector benefit from a better peer group.

[19] Another way to motivate sorting behavior is to model schools as "clubs" into which students self-select. Epple and Romano (1998) use this approach to model the allocation of students between schools.

[20] The number of students that switch to the private sector (b) is clearly an endogenous variable. The appendix illustrates how b might depend on variables that reflect the marginal benefits of private schooling in a community. We discuss this point in greater detail in the empirical section of the paper.

These expressions provide a first set of testable implications. The obvious one is that private schools should have higher average SES than public schools. The more interesting one is that since $\frac{\partial \overline{SES}^{\,c}_{pub}}{\partial b} = \frac{\alpha}{2} > 0$, the average SES of students in the public sector should decrease with private enrollment.[21]

4.3 Vouchers' effect on academic achievement

We now consider the effect of vouchers on academic achievement, distinguishing between the effect on average outcomes in the public school, among students who switch to the private sector, and on all the students in the community.

4.3.1 Public School Students

Taking as given that students $i \in [b, 1]$ switch to the private sector, the change in the average test score among public school students is:[22]

$$\Delta \overline{T}_{pub} = \beta_{pub} + (b-1)\frac{\gamma\alpha}{2} + (b-1)\frac{\lambda\alpha}{2} + (b^2-1)\frac{\phi\alpha^2}{4}. \qquad (2)$$

To assess the impact of school choice on public performance, it is useful to distinguish between three effects included in this expression:

1) The *productivity effect* of competition, measured by β_{pub}, which reflects the fact that competitive pressures may force public schools to improve.

2) The *direct effect of student composition*, measured by the second right hand side term. This expression reflects that since the average SES in public schools has fallen, all else

[21] The model has a similar implication for private schools: as they take more students formerly in the public sector, the SES of their marginal student will decline.

[22] To see this, note that before the voucher program, the average test score in public schools was simply the average score of all students in the community:

$$\overline{T}_{pub} = \frac{\gamma\alpha}{2} + \frac{\lambda\alpha}{2} + \frac{\phi\alpha^2}{4}.$$

After vouchers are introduced, only students $i \in [0, b]$ remain in public schools. Using the fact that $\overline{SES}^{\,c}_{pub}$ is $\frac{\alpha b}{2}$, we get the following expression for the average score among public school students:

$$\overline{T}^{\,c}_{pub} = \beta_{pub} + \frac{\gamma\alpha b}{2} + \frac{\lambda\alpha b}{2} + \frac{\phi\alpha^2 b^2}{4}.$$

By subtracting the first expression from the second one we get equation (2).

equal their average test score will fall as well. The simplest way to see this is to assume that $\beta_{pub} = 0$ and $\lambda = \phi = 0$, i.e., there are no productivity or peer effects. In this scenario, the change in average public test scores is $\Delta\overline{T}_{pub} = (b-1)\frac{\gamma\alpha}{2} < 0$, which simply reflects the fact that because of sorting, the public schools have lost their "best" customers.

3) The *peer group effect*, measured by the last two terms. If peer effects exist, this will adversely affect the performance of students who remain in public schools.[23]

These last two effects (the direct effect of student composition and the peer group effect) are important because they imply that if choice also leads to greater sorting then it will be nearly impossible to empirically identify the effect of competition on the public sector's productivity. To illustrate, there is recent body of work on whether public schools perform better in regions where they face greater competition, where competition is typically proxied by the private enrollment rate. The main methodological issue raised in this literature is that private enrollment shares are potentially endogenous to the initial quality of public schools, and a number of papers use instrumental variables to address this problem.[24,25]

Equation (2) suggests that as long as choice leads to greater sorting, there is a further problem with this approach. Specifically, let us put aside the issue of whether choice is correlated with the initial quality of public schools, and imagine an experiment in which school choice is randomly assigned to some communities. In this case, even if the productivity of the public schools in these communities improves, the performance of the average student in the public sector might still fall simply because the best students have moved to private schools.

[23] To see this, consider the effect on those students who remain in the public school after the voucher system's implementation. The change in test scores for this group is

$$\Delta\overline{T}_{pub} = \beta_{pub} + (b-1)\frac{\gamma\alpha}{2} + b(b-1)\frac{\phi\alpha^2}{2}.$$

Thus, vouchers have two effects on students who remain in public schools. On the one hand, their achievement rises due to the productivity effect (β_{pub}); on the other, as long as peer group quality raises test scores, their performance falls due to a decline in their classmates' "quality".

[24] Hoxby (1994) and Dee (1998) use the local incidence of catholic affiliation as an instrumental variable, and suggest that competition does improve public student outcomes; but McMillan (1997) and Jepsen (1999) have not found competitive effects using this same strategy. McEwan and Carnoy (1999) use a variant of this approach in the case of Chile, and find weak evidence of achievement gains due to vouchers. Couch, Shughart II and Williams (1993) find positive effects of competition, but Newmark (1995) finds that their results are not robust to alternative specifications.

[25] A third approach draws on Tiebout (1956) to use the extent of fiscal decentralization as a proxy for competition, see Hoxby (2000) and Urquiola (2000).

Put differently, when competition results in greater sorting, there is simply no instrument that would allow us to isolate the effect of choice on the public sector's productivity.

4.3.2 Private school students

We now turn to the gain in academic achievement for students that switch to the private sector. The change in test scores among these individuals is:

$$\Delta \overline{T}_{priv} = \beta_{priv} + \frac{\lambda \alpha b}{2} + \frac{\phi \alpha^2 (1+b)}{4}.$$

Here, there are essentially two effects:

1) A productivity gain, assuming $\beta_{priv} > 0$. In words, if private schools are more effective than public schools, these students gain simply from switching to the private sector.
2) A peer group effect. As long as the marginal effect of the peer group is positive, these students also gain from having sorted themselves into a better peer group.

Here again, there are important implications for how we should interpret the empirical evidence on the potential gains from school choice. Specifically, at the heart of choice proposals is the notion that private schools are more effective than public ones. The original and most common test for this relies on a regression like:

$$T_i = \alpha + \beta_1 P_i + \mathbf{X}_i' \beta_X + \epsilon_i \tag{3}$$

where T_i is student i's test score, X_i is a vector of socioeconomic controls, and P_i is an indicator variable for enrollment in a private (often catholic) school. A positive β_1 is interpreted as a productivity advantage. As is widely acknowledged, the main weakness of this approach is that characteristics not included in X_i may simultaneously affect students' scores and their private enrollment status.[26] To address this problem, more recent studies

[26] For instance, Hoffer, Greely and Coleman (1985), Chubb and Moe (1990), and Evans and Schwab (1995) suggest private schools are indeed more effective, while Willms (1985) finds that when enough variables are included in X_i, this finding is no longer significant. Murnane (1984), and Witte (1992) provide further discussions of this literature. In Chile, earlier studies like Rodríguez (1988) and Aedo and Larrañaga (1994) suggest that voucher schools are more effective; more recently, Mizala and Romaguera (1998) and Bravo, Contreras and Sanhueza (2000) point out this conclusion depends on the control variables used and the samples considered. McEwan (2000) finds a private advantage, but only for catholic voucher schools. For further research, see Tokman (2001).

rely on instrumental variables or arguably randomized research designs.[27]

However, note that while a randomized research design provides an unbiased answer to the question "would a student selected at random perform better in a private than in a public school?" one still cannot interpret β_1 as a pure productivity advantage. To see this in our framework, note that if we compare two identical students (i.e., with the same SES), and find that the one in private school does better than the one in public school, we cannot attribute this only to a positive β_{priv}. One has to acknowledge that the student in the private school enjoys a better peer group ($\overline{SES}_{priv} > \overline{SES}_{pub}$). To isolate β_{priv} we would also need to know λ and ϕ, even assuming that we had identified the correct functional form for peer effects.

In the absence of such information, a positive β_1 in a regression like (3) could reflect peer effects rather than any true productivity difference. Put otherwise, randomization guarantees that the children in treatment and control groups are identical along all (their own) characteristics but private enrollment status; it does not imply that their classmates' will be similar.

This matters because the overall peer quality in a given community is fixed, so if there is no true private productivity advantage, switching children between sectors could end up having no effect, or may even reduce aggregate achievement (if the peer effect-related gain for students moving to the private sector is smaller than the loss to children left behind in the public sector). In other words, even with a randomized research design, (3) cannot answer the question "what would happen to achievement if we shifted a substantial proportion of children to the private sector?" The general point here is that such regressions address a "partial equilibrium"-type question, while from a policy perspective we are interested in the "general equilibrium" consequences of choice.

This issue arises whenever the private sector is better endowed with some input, like peer group quality, the aggregate supply of which is fixed at least in some short or medium run. For an additional illustration, suppose achievement were determined only by teacher

[27] Neal (1997) instruments for catholic school enrollment, and finds a significant productivity advantage for catholic schools. Rouse (1998) examines the effect of the Milwaukee initiative and finds mixed evidence that students benefit from using these vouchers to attend private schools. More recently Angrist et al. (2001) find positive effects from a partially randomized voucher program in Colombia.

quality. Assume additionally that there are two types of teachers, effective and ineffective, a characteristic determined entirely by the type of training they receive. Finally, assume that initially the effective teachers all work in the private sector, while public schools have only ineffective instructors. In this setting, if we transfer a randomly chosen child from the public to the private sector, he will do better, and an analysis like (3) would suggest a private advantage. To see that it is incorrect to interpret this as a productivity advantage, note that if we started transferring larger numbers of children, at some point the private sector would have to hire ineffective teachers (assuming it wanted to maintain a reasonable class size), and the marginal benefit of transferring students would decline. Unless one can increase the supply of better-trained teachers, aggregate achievement is fixed in this setup.

4.3.3 All students

Finally, we can consider the effect of the voucher program on test scores among all students in the community. This is simply a weighted average of the change in scores of students $i \in [0, b]$, those that remain in public schools, and students $i \in [b, 1]$, those who switch to the private school:

$$\Delta\overline{T} = b\Delta\overline{T}_{pub} + (1-b)\Delta\overline{T}_{priv} = b\beta_{pub} + (1-b)\beta_{priv} + \frac{\phi\sigma^2 b(1-b)}{4}. \qquad (4)$$

The first two right hand side terms capture the productivity effects of the voucher program. The third expression reflects the net peer effect of the sorting induced by the program. If $\phi < 0$ (lower SES students benefit more from better peers), this last expression is negative. The intuition is that if the loss from sorting is more important for the lower SES students (who remain in public schools) than the gain to private school students from having better peers, then the net effect of changes in peer groups is negative. An important implication is that although we cannot empirically isolate β_{pub} and β_{priv} from the peer group effects, we can net out the "direct" effect of changes in each sector's student composition by measuring changes in educational outcomes among *all* students in a given community.

5 Data and coverage

The results presented below rely on several types of data spanning the 1970-1999 period: all matched at the school, commune, or national level. These include information from two national testing systems, administrative databases of the Ministry of Education, household surveys, and population censuses. Table A.1 lists the precise sources of our data, the years for which we use them, and the original unit of observation. By way of summarizing this table, we can focus on the educational outcome variables it yields.

Our first outcomes are mathematics and language test scores, which the PER testing program provides for 1982, and the SIMCE for later years.[28] Both of these systems include the 4^{th} and 8^{th}grades, but we focus on the former. We complement this information with school-level administrative data collected by the Ministry of Education for 1982 and 1988, which yield repetition rates as a second outcome variable. This also allows for greater coverage, since the PER, which is the source of our test data for 1982, did not reach several rural municipalities.[29] Because the administrative data is a census of schools, this allows us to confirm that our results with test scores are not driven by the choice of communes.

Finally, we consider the average years of schooling among 10-15 year olds as a third outcome variable. This measure captures several dimensions of the educational system's performance, since it reflects factors like age at entry, repetition, and dropout patterns. We draw this variable from census and CASEN[30] household survey micro data.

A drawback with our school-level (test system and administrative) data is the absence of detailed socioeconomic characterizations of schools. The Ministry of Education classifies each school into three to four categories of socioeconomic status, but this information is obviously rather coarse. To address this weakness, we incorporate household survey data into the analysis. In Chile, the CASEN survey collects information on the specific school children attend, and we link these data to administrative records to obtain detailed information on the socioeconomic profile of individual schools.

[28] PER stands for *Programa de Evaluación del Rendimiento Escolar*, and SIMCE for *Sistema de Evaluación de Calidad de la Educación*. These tests have been conducted every year (with the 4^{th} grade in even and the 8^{th} in odd years) since 1982, with a suspension during 1985-87.

[29] Nevertheless, it still reached about 90 percent of all students, see Espínola (1993).

[30] *Encuesta de Caracterización Socioeconómica Nacional.*

Table 2 presents school level summary statistics for the years 1982 and 1988. We focus on the 1980's because this is the period in which the voucher reforms are likely to have had their largest effect. In addition, table 3 contains summary statistics at the commune level. These are the geographical units we use as proxies for educational markets. Chile has approximately 320 communes (depending on the year considered), and they have a median area of about 55 square kilometers and an average population of 39 thousand. In 1988, the average commune had 27 schools, 18 of which were public. Of the remainder, 7 were private voucher and 2 were tuition-charging private institutions. Each commune has an autonomous government that manages public schools and other public services. Metropolitan Santiago contains about 50 such units.

The results below consider two sets of markets: i) all communes, and ii) a restricted sample that includes only those with populations above 10 thousand, urbanization rates over 80 percent, and locations outside the greater Santiago area. We created this second set for two reasons. First, communes with low populations and/or urbanization rates generally contain sparsely populated areas in which students cannot easily commute, and private sector participation is rare. Second, communes in the greater Santiago region are relatively small and clustered, and thus are less likely to be independent markets, since children are able to cross commune lines to attend both public and private schools. None of these restrictions alter the nature of the conclusions reached below.

6 Results

This section explores the implications of the model we developed earlier. Because sorting is by assumption the key engine in this framework, we first use a variety of approaches to explore the extent to which the introduction of vouchers has resulted in increased between-sector segregation in Chile. We then measure the effect of vouchers on achievement, focusing on the effect on aggregate educational outcomes in entire educational markets.

6.1 Sorting: cross sectional evidence

Our model's central implication is that the public sector's relative socioeconomic quality declines with private enrollment, as it loses its "best" customers to the private sector.[31] All else equal, its relative test scores should decline as well. Figure 6 presents prima facie evidence supporting the latter prediction. It uses cross-sectional data for urban, non-Santiago communes. The graph refers to 1990, but the result it illustrates holds in any year among those considered below.

On the figure's x-axis is the total private enrollment rate (including both voucher and tuition-charging private schools), which ranges from 0 to about 0.60 in this sample. The y-axis plots the expression $\frac{\bar{T}^c_{pub}}{\bar{T}}$, the ratio of the (enrollment-weighted) average language score in public schools and the average score in all schools, where both the numerator and denominator are commune-specific. Each circle denotes a commune, and each circle's size is proportional to the commune's population.[32] Thus, the figure addresses how the *within-commune* relative public performance varies with the private enrollment rate.

As expected, the intercept of the relation described is one: in areas without private enrollment, the average public score corresponds to the aggregate average. In keeping with the model's prediction, as private enrollment rates increase, the relative performance of public schools declines, a relation that is highly significant. An R^2 of 0.65 for this bivariate relationship suggests that the private enrollment ratio is indeed a very good predictor of the public/private gap.

Recall that according to expression (2) (page 13), private enrollment would have three consequences on the public sector's relative performance: i) a productivity effect, which would tend to improve it, ii) the direct effect of student composition, which we refer to as sorting and which would adversely affect it, and iii) peer group effects. We interpret figure 6 as suggesting that the second of these is an important factor: whatever improvements the public sector's productivity experiences due to competition seem to be overwhelmed by the loss of its "best" customers. The next few tables and figures provide additional evidence in

[31] Formally, $\frac{\partial \overline{SES}^c_{pub}}{\partial \theta} > 0$.

[32] In keeping with the above discussion, the communes to the left, those with low private enrollment, tend to be smaller.

support of this interpretation.

To begin, table 4 presents further cross-sectional evidence. All the regressions refer to urban, non-Santiago communes; the results obtained using all communes are qualitatively similar. The first column in panel A simply replicates the bivariate specification in figure 6, in this case for math scores. This simple specification accounts for 51 percent of the variation in relative public school performance. Regression 2 adds commune level controls and regional dummies to the specification. This essentially does not alter the point estimate of the effect of private enrollment, which remains statistically significant at the 1 percent level. These regressions suggest that private enrollment has fairly large effects on relative public performance: a one standard deviation increase in the private enrollment ratio increases the relative gap by about 75 percent of a standard deviation.

Columns 3 and 4 present similar information using repetition rates as an alternate outcome variable, where these measure the proportion of students who were not promoted at least twice.[33,34] In this case, a positive coefficient implies worse performance, so the results point to the same conclusion: increases in the private enrollment ratio are associated with a worsening relative public position. The effect sizes and significance levels, however, are somewhat smaller than with test scores.

Instead of looking at educational outcomes, panel B focuses directly on SES. Columns 1 and 2 use information on parents' schooling from the testing systems. Despite the fact that these data are crude, the results suggest that the relative educational attainment of parents in public schools is lower in communes with higher private enrollment levels. Working with richer data, columns 3 and 4 include regressions using SES information from the CASEN household survey (matched to schools). The results are similar but the significance levels lower, which in part reflects that the CASEN does not provide information on every school.

To summarize, table 4 uses different types of data to underline a robust cross-sectional result in Chile: the public sector's relative performance seems to be worse in areas with more private enrollment. This finding is consistent with sorting of the type modeled above, that

[33] This is the definition of repetition used by the Ministry of Education.

[34] As detailed earlier, repetition rates have the advantage of being drawn from a census of schools, and are collected independently from the test scores considered in regressions 1-2.

is, with a situation in which the voucher private sector, as it expands, picks up the "best" students from public schools.

6.2 Sorting: fixed effects

This interpretation could be erroneous, however, if there are factors other than sorting that affect both relative public performance and private enrollment rates. For instance, it could be that public schools in certain communes are particularly ineffective, making it easier for more efficient private schools to enter the market and compete. This could result in data like those in figure 6, even if voucher schools were not "cream skimming".

As a first way to address this issue, table 5 presents similar evidence using within-commune, over time variation in private enrollment for the 1982-1988 period, during which the voucher program had its strongest effects. Panel A begins with a sample restricted to urban-non Santiago communes covered in both the PER and SIMCE testing systems.[35]

Focusing on relative performance in math as the key dependent variable, column 1 includes only a constant and a 1988 dummy in the specification. The coefficient on the latter is highly significant, illustrating that across the communes considered, the relative performance of public schools declined in these six years. Specification 2 adds private enrollment, and column 3 includes control variables at the commune level. These suggest that most of that decline can in fact be accounted for by increases in private enrollment, and that this result is robust to the addition of several controls. Finally, column 4 includes commune dummies so that the effect of private enrollment is identified using only within-commune variation. These results confirm the inference that public school performance fell by more in communes with a larger increase in the private enrollment share, although the point estimate is significant only at the 10 percent level. Columns 5-8 present similar evidence for Language scores, and in this case the effect is larger and remains significant at the 1 percent level throughout. In panel B, columns 1-4 use direct measures of socioeconomic status, and finally, columns 5-8 show that these results are confirmed using repetition rates as the outcome variable.[36]

[35] The lower coverage of the PER accounts for why data in table 4, which uses only SIMCE data, refer to 48 communes, while in table 5 the (stacked) data concern 42. The conclusions from table 4 would be the same if the sample were similarly restricted.

[36] Where, once again, a positive coefficient implies worsening performance.

To summarize, these results show that to the extent that sorting is illustrated by an association between increasing private enrollment rates and worsening relative public performance, it is a feature not only of cross sectional analyses but also of fixed effects specifications that exploit the effects of the 1981 voucher reforms in Chile.

6.3 Sorting: evidence from entry patterns

Moving on to a final way of assessing the presence of sorting, recall our model suggests that an entering voucher private school would "cream skim" the best students from the public sector. It is straightforward to extend the model to show that similar behavior would take place if there were sequential entry. The first entrant into a market previously dominated by a public monopoly would pick the best students up to some size dictated by, say, minimum efficient scale. The next voucher school would pick up the next best group, and so on.

From an empirical standpoint, this logic implies that a given point in time, the SES of incumbent voucher schools should be higher than that of entrants, and that according to the previous results, we should observe similar behavior in their test scores. Figure 7 compares the distribution, by SES category, of incumbent and entrant voucher private schools in 1988.[37] As expected, the proportion of entrants in the lowest SES category is much larger, while incumbents are relatively more represented in the other, particularly the two highest, categories. Figure 8 presents similar evidence for test scores, showing that, on average, the incumbents outperform the entrants.

6.4 Measuring productivity effects of choice

Thus far, our results suggest that increased sorting has been an important consequence of Chile's school voucher system. In the context of the school choice literature, this finding is interesting because of concerns over the impact of choice on inequality, a topic which we address later. It also suggests that the proper manner to assess whether vouchers resulted in achievement gains is to measure changes in educational outcomes at the *aggregate* level.

[37] Here the incumbents are schools that exist in 1988 and were already in operation in 1982, and the entrants are those that appeared between the two years, so that we observe them only in 1988. This figure uses information only on communes with test data for both years, all of which are predominantly urban.

Further, it suggests that the two approaches generally used to measure the potential achievement gains from school choice would tend to yield opposite inferences on these benefits . On the one hand, comparisons of public and private schools would suggest that private schools are better; on the other, estimates of how competition (as measured by private enrollment rates) affect public performance would suggest that choice hurts public performance. Before proceeding to estimating outcomes at the aggregate level, this section shows that these predictions are confirmed in Chile, and places this paper in the context of previous empirical analysis in this country.

As a first step, Table 6 presents standard comparisons between public and private scores, using school level data. This type of regression is the most common in the literature, and tries to explain schools' (or students') performance using a series of characteristics and a private sector dummy. This table illustrates an enduring feature of this type of analysis in Chile (and many other countries): a positive and significant coefficient on the private dummy, which persists even when one adds school and commune controls, and in this case, commune and regional dummies. This is the case even though the regressions include only municipal and voucher private schools, which have similar expenditure levels; the advantage is larger if one adds tuition-charging institutions, or if one focuses only on the urban commune sample considered in previous tables. These results are similar to those obtained in earlier work, and account for the generally positive assessment of vouchers produced by private/public comparisons in Chile.[38] It is only the analysis with more recent versions of the SIMCE, which has individual-level information and more substantial SES data, that finds that the private advantage disappears when "sufficient" controls are included.[39]

Moving on to the effects of private enrollment on public school performance, table 7 presents results using the 1990 SIMCE. Column 1 describes the simplest univariate specification, which suggests private enrollment in fact raises public schools' test scores. In table 1 we showed, however, that higher income, more urban communes tend to have higher private enrollment rates, which could account for this finding. To address this fact, regressions 2-4 add a series of school and commune level controls, the same ones used in table 6. Once this

[38] See for instance Rodríguez (1988) and Aedo and Larrañaga (1994).

[39] See Mizala and Romaguera (1998), Bravo et al. (2000), and McEwan (2000).

is done, the results suggest that the extent of private enrollment has a significant negative effect on public school performance, as predicted in Section 4.

To summarize, tables 6 and 7 are consistent with the type of sorting modeled above. We note that they do *not* necessarily imply that there is no private productivity advantage, or that competition does not raise public schools' productivity. They merely suggest that sorting-induced composition effects are relevant to the analysis of the productivity consequences of school choice.

6.5 Vouchers: effects on average achievement

The model introduced in this paper suggests that if one wants to identify productivity effects, the way to control for the direct effect of sorting is to consider how vouchers affect performance in entire educational markets. As a reminder to the reader, this measure is the sum of the two sources of productivity gains from choice and the net effect of sorting on peer groups.

To begin this analysis, table 8 presents cross sectional regressions for all communes and the sub-sample of urban non-Santiago communes. We use three measures of educational outcomes: math scores, repetition rates, and the average years of schooling for children aged 10 to 15 (panels A, B and C, respectively). The results all point to a uniform conclusion. In the simplest specification, we find that communes with a larger private enrollment share have higher test scores, lower repetition rates, and more years of schooling among 10-15 year olds. Once rudimentary commune-level controls and regional dummies are added, the results are no longer significant, and most point estimates suggest that communes with a larger private enrollment share in fact have worse aggregate educational outcomes.

Turning to fixed-effect evidence, table 9 explores whether average outcomes in communes with large increases in private enrollment after 1982 have improved relative to communes in which the voucher program had a smaller impact. As before, we use three outcome measures for two sets of communes. The results also provide no evidence that the voucher program improved outcomes. If anything, although they are not statistically significant, the point estimates suggest that increases in private enrollment worsened average performance. In

communes with large increases in private enrollment after 1982, average math scores fell (in one of the two samples), repetition rates increased, and average years of education among 10-15 year olds declined relative to communes with small increases in private participation.

We note that the results in table 9 do not imply average educational outcomes did not improve at all in Chile, merely that whatever improvement there was does not seem to have been any greater in areas where private participation grew more. Specifically, while the table does suggest that average test scores did not change (although caution is always warranted in comparing absolute test scores over time) the 1988 dummies show overall improvements in repetition and mean years of schooling. Such improvement is to be expected, if only because of the rapid income growth Chile experienced during this period.

In sum, we find no evidence that school choice resulted in better educational outcomes, at least not on the scale claimed by some of its advocates. One could still argue that the voucher program in Chile resulted in higher school productivity, but that the effect of enhanced productivity on average outcomes is overwhelmed by the distributional impact of sorting. Specifically, in our model, equation (4) (page 17) raises the possibility that average outcomes may not change ($\Delta T = 0$) even if there are positive productivity effects ($\beta_{pub}, \beta_{priv} > 0$), provided that the loss from sorting is more significant for the lower SES students who remain in public schools than the gain for the higher SES children who, having moved to the private sector, come to enjoy a better peer group.

Although this is possible, if the productivity gains from vouchers are as large as is often suggested, it seems difficult to believe that the net peer effects could be large enough to mask them. Nonetheless, to address this possibility, the next part of the paper turns to an analysis of the effect of the voucher program on the distribution of educational outcomes.

6.6 Vouchers and the distribution of achievement

Thus far, we have focused on the effect of vouchers on average achievement. However, the exodus from public schools could also have a significant effect on the distribution of school outcomes. On the one hand, students who use vouchers may benefit from attending private schools. On the other, a voucher-induced exodus of public school students could adversely

affect those left behind in worsened peer groups.

As a first piece of descriptive evidence, figure 9 plots the 1990 residuals of regressions of math test scores on individual characteristics for urban, non-Santiago communes.[40] The figure displays the distributions of these residuals according to communes' private enrollment level, and under the assumption that the average score in each school is representative of all the students enrolled.[41] The horizontal lines represent the fitted values of quantile regressions for the 95th, 75th, 50th, 25th, and 5th percentiles of students in each commune. The regression line for the median student is essentially horizontal, reflecting the fact that average test scores are the same across communes with different private enrollment shares (the result explored in the previous section). There is, however, suggestive evidence of a "fanning out" of academic outcomes: the gap between the best and worst students is wider in communes with more private participation. We also find similar patterns for languages scores and repetition rates.

Caution is warranted in interpreting this evidence, since the results are for a cross-section of communes in 1990, and the dispersion of test scores across communes may not be entirely due to changes induced by the voucher program. As a partial solution, we turn to micro-data from the 1982 population census and the 1990 and 1992 CASEN household surveys to explore whether the educational outcomes of children from low socioeconomic backgrounds have worsened over time in a manner that might be associated with the voucher program. We use years of schooling among children between the ages of 10-15 as our outcome measure, and classify children into four socioeconomic groups based on their parents' education.[42] With these variables, we measure whether the change in years of education (from 1982 to 1990-1992) of children in the four socioeconomic classes in each commune is correlated with the change in the private enrollment share in the commune.

The results are shown in table 10. For the complete sample of communes, column 1

[40] The figure's central characteristics are similar if raw math or language scores are used instead.

[41] Student-level data is necessary to fully implement this analysis. Because we do not have such data for the years of interest, this figure assumes that there is no within-school variance in test scores. We do have individual-level data for the 8th grade in 1997, and the qualitative conclusions are similar in that case.

[42] The four categories are children whose parents have less than five years of schooling (29 percent of all children 10-15), between six and eight years of schooling (26 percent), between nine and 12 years of schooling (31 percent), and 13 or more years of schooling (14 percent).

simply includes the group dummies (excluding that for group 3), producing the expected ordering. Column 2 interacts these dummies with one for 1990/92 observations, showing that average educational attainment increased for all these groups. Finally, to study the effects of private enrollment, specification 3 adds interactions of the group dummies and the change in private enrollment over the 1982-1990 period. Although these last estimates are generally not statistically significant, the point estimates provide some evidence of a greater dispersion in academic outcomes in communes where the voucher program had a large effect. They indicate that in a commune where the private enrollment share increased by 20 percent, the average years of schooling of 10-15 year old children from the lowest socioeconomic class fell by an average of 0.5 years (-1 x 0.2) relative to that of similar children living in communes where the voucher program had no effect.

This evidence of a widening gap in educational attainment is important because a central question in the voucher debate is whether children from disadvantaged backgrounds would suffer from the mass departure of wealthier students from public schools. Nonetheless, when stacked against the vaunted productivity gains from choice, the extent of the widening gap seems small, certainly too small to explain how average educational outcomes could remain unchanged if in fact choice produced large productivity improvements.

6.7 Possible biases

The results presented thus far rely on between-commune variation in private enrollment rates. To address the usual problems that arise with such cross-sectional evidence, this paper has also used within-commune changes in private enrollment between 1982 and 1988, the years in which the voucher program had the largest impact. While this procedure has the advantage of controlling for those commune characteristics fixed over time, it too can yield biased estimates of the extent to which vouchers have improved school outcomes.

To begin, we remind the reader of the evidence presented earlier (in table 1) that the voucher program had a larger effect in urban, populated, and wealthier communes. There are reasons to believe that the marginal returns to private education would be relatively high in such communes. What biases might this induce? Because our central estimates

of the impact of choice compare the change in average academic achievement in communes where the marginal returns to private education were high, to communes in which these were relatively lower (and in which the introduction of choice led to a smaller increase in private enrollment), it is likely that these estimates are higher than those that would answer the question "what would be the effect on academic achievement of increasing school choice in a randomly selected community?" Put differently, our estimates measure the average *marginal* effect of school choice on aggregate educational productivity, which are probably *higher* than the *average* effect of school choice on productivity.[43]

In addition, our discussion suggests that the voucher program would also have a larger effect in communities in which public schools are under-performing (this is formally illustrated in the appendix, equation 5, page 37). If this were the case, this would also imply that our estimates of the impact of the voucher program on achievement are upwardly biased estimates of the average impact of vouchers. To check for this, we use two measures of the performance of schools serving "middle-class" families in each commune: (1) the 1982 average test score of children in "middle-class" public schools relative to scores of children in "middle-class" private schools; and (2) the fraction of 13 year-olds with completed primary education from "middle-class" families relative to all 13 year-olds in the commune.[44] These regressions, shown in columns 1 and 2 in table 11 (panel A), provide some support for the hypothesis that private entry was higher in communes where the public sector was under-serving the middle class. Although the explanatory power of these variables is quite low, accounting for less than three percent of the cross-commune variance in the growth of private enrollment, they provide additional evidence that if anything, our estimates of the effect of the voucher program on aggregate achievement are upwardly biased.

However, if public schools were not only under performing in communes where private enrollment grew rapidly after 1982, but also if their performance was *falling* over time in such communes, then our estimates could be downwardly biased, since such a pre-existing trend would mask voucher-related productivity gains. In essence, the voucher program would have

[43] See Angrist and Imbens (1995).

[44] In the first case, "middle class" refers to the middle of three SES categories the Ministry of Education used to classify schools in 1982; in the second, it refers to children from households where the household head has at least a primary education, but is not a high school or college graduate.

served to "level-up" those areas with declining performance, but we might still not estimate any positive effects on average achievement. Although we do not see evidence of pre-existing trends at the nationwide level, panel B in table 11 explores whether they were there at the commune level, using four measures of educational outcomes at the commune level.

Regressions 1 and 2 begin with an indirect approach, checking whether communes in which the private enrollment rate was growing prior to 1982 also had large increases in private participation after 1982. The logic here is that as a reaction to declining public performance prior to 1982, households may have started moving to the private sector even before the introduction of vouchers. Thus, evidence of a positive correlation between 1980-82 and 1982-88 changes in the private share might indicate pre-existing declines in performance. The first univariate regression shows, however, that these two measures are unrelated; this is not simply an issue of statistical insignificance — the point estimate is essentially zero.

We would have liked to have commune-specific private enrollment rates from a year even prior to 1980, but were unable to obtain these. As a partial solution, regression 2 focuses on the proportion of *schools* in each commune that were private, checking whether the 1978-1982 change in this measure is correlated with the 1980-1982 changes in private enrollment.[15] Once again, we find no evidence that public schools were worsening in communes where the voucher program had a large effect.

Regressions 3 and 4 in panel B turn to a more direct test of whether educational performance was declining in communes where the private sector grew most after vouchers. In this case, the independent variable, based on the 5 percent sample of the 1970 and 1982 population censuses, is the 1970-1982 change in educational attainment among all 10-15 year olds in the first case, and among "middle class" 10-15 year olds in the second.[16] Here we find strong evidence that educational outcomes were improving, not declining, in communes that experienced a greater increase the private sector's share. In fact, this might be what one would expect given private voucher schools' preference to locate in more urban, wealthier communes. This reinforces that our previous results may overestimate voucher-related

[15] We manually collected the school counts from maps, data source (18) in table A4.

[16] We define "middle class" children as those from households where the household head has at least a primary education, but is not a high school or college graduate.

productivity effects.

Finally, by looking at *concurrent* trends, panel C considers a different type of bias that might be present in our estimates. Specifically, it focuses on 1982-1990 changes in wealth, proxying this variable with the percentage of households that own their homes (column 1), and with the percentage that have telephone connections (column 2). We find, as our model and anecdotal evidence would lead us to expect, that voucher schools have grown more in areas where such proxies have also made further gains. Once again, this suggests that our results are overestimating vouchers' effects on achievement, since some of the improvement in outcomes such as repetition rates may be due to income growth.

6.8 Vouchers and performance: Aggregate evidence

A skeptical reader may still not believe our finding that choice had no effect on aggregate achievement, perhaps due to doubts about the causes of the differential impact of the voucher program across communes in Chile. To hopefully convince such a person of the robustness of our findings, in this section we present "macro" evidence on changes in academic achievement in Chile, evidence which does not rely on cross-commune variation.

Our first piece of evidence is from 1970 and 1999 international tests in science and mathematics, widely known as the TIMSS. As previously discussed, the data (shown in figure 1) show that between these two years, Chile's performance in these test has not improved relative to the other 12 countries that also participated in both exams; in fact, Chile moved back one position in the 13 country ranking. This is very surprising for at least three reasons. First, as previously discussed, Chile's economy has grown at a faster rate than that of the other countries. Second, real per-student educational expenditures in Chile have increased by over 130 percent over this time period.[47] Third, this was not a period of rapid increases in enrollment rates, so the results cannot be attributed to an expansion in Chile's educational system.[48]

[47] The change in average educational expenditures is from 1970 to 1996, and is calculated from Programa Interdisciplinario de Investigaciones en Educación (1984), and the Ministry of Education's 1996 *Compendio Estadístico*.

[48] According to our tabulations from the 1970 census, the 1982 census, and the 1998 CASEN household survey, the enrollment rate among children in the 7-12 age range increased from 93.7 percent in 1970, to

One might still be concerned about the validity of this comparison, because most of the other 12 countries are developed nations. Nevertheless, as illustrated in figure 10, Chile also ranks near the bottom among the 38 countries that participated in the 1999 TIMSS, under-performing other developing countries with similar or lower per capita incomes. To further explore this, Figure 11 presents residuals from a regression of the median score on log GDP per capita, the pupil-teacher ratio, and the net enrollment rate (using data at the country level). If vouchers had large effects on school productivity, we would expect Chile to have a positive residual, but again this is not the case.

Finally, returning to within-Chile evidence, we can also use nationwide average test scores to gauge the extent to which aggregate academic achievement has improved. It would obviously not make much sense to plot the time trends in the average test scores, due to comparability issues. However, since the tuition-charging private schools were largely unaffected by the voucher program, we can use the gap between these schools and the rest (voucher and public schools) as a measure of changes in aggregate academic quality. The idea is that in the same way that other countries can be used as controls in the TIMSS, we can use tuition-charging private schools as a control for the PER and SIMCE exams.

This evidence, presented in figure 12, also provides no indication that vouchers have improved outcomes in the schools they affect. In this figure, the data for 1982 show a well known feature of the Chilean education system: there is a large test score gap between the subsidized sector (voucher and municipal schools) and the tuition-charging private schools; in this case the subsidized scores are about 1.3 standard deviations below the elite private schools. The figure also shows that by 1996, this gap had actually become somewhat larger.[49]

6.9 When schools compete, how do they compete?

To recapitulate, we find that sorting has been the central effect of school choice in Chile, and that competition has not resulted in overall achievement gains. While it might seem plausible that choice increased segregation, our finding that it had no effect on aggregate

97.4 by 1982, and 98.9 in 1998.

[49] In part, this in itself may be capturing some sorting, since the tuition-charging private sector did grow significantly (although from a small base) during this period, presumably "cream skimming" some students from voucher and even municipal schools.

educational outcomes is more surprising. What could account for this result?

One possibility, often raised by Chilean observers, is that public schools may in fact not have experienced significant incentives to compete. We presented prima facie evidence consistent with this critique in figure 4, which showed that few public schools have been forced to close.[50] As our model made clear, however, even if the public schools were not forced to compete and thus did not improve, as long as private schools have a productivity advantage, we should still see better aggregate performance given the large number of students that have transferred to the private sector, and we simply find no evidence of this.

So what can account for the lack of a private productivity advantage, and for private schools' apparent failure to improve their own achievement, given that they were clearly exposed to competition? Our view is that private schools responded to the competitive pressures unleashed by the voucher program, not by raising their productivity, but rather by choosing better students. School administrators in Chile, as in the rest of the world, can raise their schools' outcomes by doing things such as hiring good teachers and supporting and monitoring their work; but they also realize that this is costly and may not always work. In contrast, it is easier to improve outcomes simply by picking the best students. Parents are also willing participants in this, and their demand for good peer groups for their children obviously reinforces the desire of school administrators to "cream skim".

When faced with the possibility that schools compete by choosing better students rather than by raising their productivity, academics should not react with the shock of the inspector confronted with evidence of gambling in Casablanca. It seems clear that elite universities worry at least as much about selecting their students as about improving their professors' teaching skills, or making their in-class performance a central criterion in performance evaluations. This may well be optimal from these institutions' perspective, but the fact remains that student selection is one of the key margins the best universities compete on. It seems hard to expect for-profit institutions (like many voucher schools in Chile) to do otherwise.

[50] There is also some evidence that the central government in some cases covered the educational deficits experienced by municipalities' education departments during the first few years of the reform, but these deficits were not very large. For example, the municipal education sector's deficit averaged 4 percent of its total budget in 1985 (calculated from Gauri, 1996). In addition, after 1988, the central government ended these subsidies.

In fact, there is abundant institutional evidence that in Chile, private schools do compete by attempting to select better students. As previously mentioned, private schools are allowed to reject students, and Gauri (1998b) presents evidence that the majority of them do exercise this ability, and that they screen children either by requiring a parental interview, or by using admissions tests. Chilean observers have also pointed out that new voucher schools have sought to attract students by endowing themselves with "symbols" previously associated only with elite, tuition charging institutions, such as uniforms, and the use of foreign and particularly English names.[51]

7 Conclusion

Economists and policymakers have long sought to understand the educational production process by which society formally transmits knowledge to future generations. Key research in this realm has argued that "money doesn't matter" because of inefficiencies, and that school choice, by aligning incentives correctly and eliminating waste, could greatly improve educational productivity and achievement.

This paper makes two contributions to this research. First, we argue that if choice induces greater segregation, then to evaluate its effects one should study changes in outcomes at the aggregate level. In addition, we argue that the general equilibrium effects of unrestricted choice are likely to be substantially different from those that arise in small-scale experiments.

The second contribution is to focus on a country that implemented nationwide school choice. We show that the first order consequence of the voucher program in Chile was middle-class flight into voucher private schools. We also show that this shift does not seem to have resulted in achievement gains, certainly not of the magnitude claimed by some choice advocates. Even if middle class children benefited from moving to the private sector, this seems to be balanced by the negative effect on low SES students who have largely remained in public schools. In some sense, the ensuing segregation is eerily similar to the history of American cities over the last few decades, as many upper and middle income families relocated to the suburbs leaving behind decaying inner cores.

[51] See Espinola (1995).

This is not to say that vouchers might not have produced any gains at all. It might be the case, for instance that after twenty years of choice, Chilean schools are spending their money in ways that parents value more. For instance, they may now be emphasizing freshly-painted walls more than reduced teaching loads. Additionally, many families surely value the availability of subsidized religious instruction. In short, school choice might improve parents' utility even if it does not improve academic achievement. But then the debate over the benefits of school choice should be framed in such terms, and vouchers should not be "sold" with the promise of large achievement gains, or as a panacea for all the ills of the educational sector.

Finally, we want to be clear that we are not claiming that incentives do not matter in the educational industry. On the contrary, we interpret the Chilean experience as providing strong support for the notion that schools do respond to incentives. The key question is incentives for what? It seems that if schools are provided with incentives to improve their absolute outcomes, and are also allowed to choose their student body, they are likely to respond by attempting to select better students. Again, this should not be surprising to those familiar with elite universities, since an integral part of the perceived quality of these institutions is their ability to "skim" the very best students. While there are enormous rewards for the institutions that are successful in this endeavor, from a societal perspective it may be a zero-sum game, since one school's selectivity gain is another's loss. We conclude that an important topic for further research is the design of alternative mechanisms that would preserve the competitive effects of vouchers, but force schools to improve by raising their value added, and not by engaging in rent-seeking behavior.[52]

[52] See for instance Epple and Romano (1999).

8 Appendix

8.1 Sorting due to school conduct

The first mechanism assumes that private schools can pick their student body, but public schools have to accept all students who wish to enroll. We assume that for private schools, the marginal cost of an additional student varies according to her SES. Specifically, we assume that the marginal cost of educating student i is:

$$MC_i = c - \delta \cdot i$$

with $\delta \geq 0$, i.e., it ranges from $c - \delta$ for the highest SES student, to c for the lowest,[53] implying that private schools will want to admit a class with the highest possible SES.

Moving on to their objective function, we will assume private schools maximize profits.[54] For simplicity, suppose that the size of the market (relative to the cost of running a school) is sufficient to support only one private school.[55] Since it can pick its students, and higher SES students are cheaper to educate, the private school will admit the highest SES children, say those indexed by $i \in [b, 1]$.[56] Assuming, as in the Chilean system, that the private school gets a lump-sum payment P for every student it enrolls, the cutoff SES level b is pinned down by the profit maximizing condition. Supposing the private school enrolls all students $i \in [b, 1]$, its profits are:[57]

$$\pi = \int_b^1 P \cdot di - \int_b^1 (c - \delta i) \cdot di$$

and the profit-maximizing cutoff level for b is

[53] There are numerous ways to motivate this cost function. There could be a tradeoff between inputs (or effort) on the part of the school and a student's SES. It could also be the case that schools know that parents care about achievement, but they also know parents have imperfect information as to how much of it is due to SES, and how much reflects schools' productivity. With this informational asymmetry, (the level of) a school's score becomes important, and it will be rational for administrators to maximize their student body's SES and thus their institution's score.

[54] This is reasonable in Chile, where many voucher schools are operated as profit-making enterprises.

[55] It is straightforward to allow for several private schools. As shown by Epple and Romano (1998), when this is the case, they will be organized in a strict hierarchy of schools (in our case, ranked by SES).

[56] We know that those admitted will choose to attend, since households want to maximize their children's peer group quality. Students with low SES (lower than b) also want to go to the private school, but will not be admitted.

[57] Introducing a fixed cost would not change the optimal cutoff level for b.

$$b = \frac{c - P}{\delta}$$

and $(1 - b)$ is the private school's market share. b will be positive as long as $c > P$. Notice that $\frac{\partial b}{\partial P} = -\frac{1}{\delta}$: as the voucher payment increases, the private school market share increases (a fall in b means that the private school enrolls more students). Similarly, $\frac{\partial b}{\partial c} = \frac{1}{\delta}$: as the marginal cost of educating a student increases, the market share of the private school falls.

8.1.1 Sorting due to comparative advantage of private schools

A second mechanism that would generate sorting is based on the idea that high SES students benefit more from attending private schools. Specifically, for student i, the benefit of attending private relative to public school is

$$benefit_i = \beta_{priv} - \beta_{pub} + \frac{\alpha\lambda}{2} + \left(\frac{\theta_{priv}\alpha^2}{2} + \theta_{priv}\alpha\right) i$$

As long as $\left(\frac{\theta_{priv}\alpha^2}{2} + \theta_O\right) > 0$, high SES students benefit more from private schooling. We will assume this condition holds. In addition, suppose that switching schools entails a fixed cost F. High SES students will use vouchers to attend private school since their gain from switching to a private school exceeds the cost, but low SES students will remain in the public sector. The marginal student (person $i = b$) is indifferent between staying in the public sector or paying the cost to switch. By equating F with $benefit_i$, we obtain an expression that pins down the private share:

$$b = \frac{\left[F - (\beta_{priv} - \beta_{pub}) - \frac{\alpha\lambda}{2}\right]}{\left(\frac{\theta_{priv}\alpha^2}{2} + \theta_O\right)} \tag{5}$$

An interesting implication is that the private enrollment share will be larger (b will be smaller) in markets with relatively bad public schools, an aspect we analyze in the paper.

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Table 1:
Explaining the 1982-1988 change in commune private enrollment rates

1982 levels of:	Dependent variable: 1982-1988 change in private enrollment[1]		
	(1)	(2)	(3)
Average years of schooling among hhld. heads with children	0.025***		0.017**
	(0.003)		(0.006)
Urbanization rate		0.083***	0.018
		(0.021)	(0.033)
Population (hundreds of thousands)		0.059*	0.030
		(0.031)	(0.032)
Population squared		-0.007	-0.002
		(0.013)	(0.006)
N	248	248	248
R^2	0.223	0.215	0.237

*, **, ***: significant at the 10, 5, and 1 percent level, respectively.

[1] The dependent variable is based on administrative information, data sources (8) and (9) in table A.1. The control variables come from 1982 census information, data source (16).

Table 2:
Descriptive statistics at the school level 1982 and 1988

Type of data	1982			1988		
	Type of school:			Type of school:		
	Public	Voucher	Private	Public	Voucher	Private
Panel A: Test score data[1]						
Math score: mean	50.8	55.4	72.2	47.0	52.9	72.4
standard deviation	9.3	11.5	8.5	9.5	11.0	8.9
Language score: mean	55.8	60.9	77.3	48.1	56.1	77.5
standard deviation	8.9	10.9	7.8	10.9	12.9	8.9
% of schools that are: high SES[2]	1.1	5.7	39.8	0.0	2.8	59.9
middle SES	43.3	57.7	57.1	5.9	29.2	40.1
low SES	55.6	36.6	3.1	61.5	52.8	0.0
lowest SES (1988 only)				32.6	15.2	0.0
Mean 4^{th} grade enrollment				41.5	47.7	39.8
Observations	1,539	546	226	3,567	1,545	364
Panel B: Administrative data[3]						
Enrollment: mean	275	239	204	218	261	179
standard deviation	359	315	254	289	319	246
Repetition rate: mean	12.4	8.6	1.7	8.7	6.2	1.2
standard deviation	8.8	8.6	3.4	7.1	7.2	3.1
Observations	6,152	1,793	527	5,810	2,336	698
Panel C: Household survey data[4]						
Household head: mean years of schooling				7.2	8.6	10.3
Mean hhld. income (thousands of pesos)				167.0	213.9	464.7
Observations				2,359	1,356	773

[1] Data sources (1) and (2) in table A.1.

[2] In the test score data, SES controls are simply sets of dummies that divide schools into categories according to parental characteristics.

[3] Data sources (8) and (9).

[4] These statistics are for the pooled 1990/92 CASEN samples, data sources (10) and (11). These school characteristics are calculated using the actual answers of household survey respondents, matched to the schools their children attend.

Table 3: Descriptive statistics at the commune level – 1982 and 1988

	1982				1988			
	Type of data				Type of data			
	Test score		Administ. or census		Test score		Administ. or census	
	Mean	N	Mean	N	Mean	N	Mean	N
Average years of schooling among 10-15 year olds[1]			5.58	150			6.33	150
Average years of schooling among "middle class" 10-15 year olds[1]			6.01	150			28.9	181
Percentage of households (with children 6-18) with telephones[1]			4.1	181			28.9	181
Percentage of households (with children 6-18) that own their own home[1]			66.3	119			59.8	119
Population (in thousands)[2]			33.7	334			38.9	334
Urbanization rate[2]			56.6	334			53.8	334
Average among public schools / *Average among all schools*:								
Math score[3]	0.967	132			0.978	313		
Repetition rate[4]			1.073	316			1.092	322
SES[3]	0.951	132			0.964	313		
Income[5]							0.857	216
Parents' years of schooling[5]							0.936	216
Private enrollment ratio: Primary (voucher and tuition-charging)[4]			0.119	316			0.176	326
Primary (voucher only)[4]			0.134	316			0.192	326
1th grade (voucher and tuition-charging)[4]					0.192	315	0.179	326
1th grade (voucher only)[4]					0.209	315	0.194	326
No. of schools: Total[4]	17.3	132	26.8	316	17.4	315	27.1	326
public[4]	11.6	132	19.5	316	11.3	315	17.8	326
voucher[4]	4.1	132	5.7	316	4.9	315	7.2	326
private[4]	1.7	132	1.7	316	1.2	315	2.1	326

[1] For 1982, these variables come from census micro data, data source (15) in table A.1. For 1988, they are constructed from 1990 and 1992 household survey information, data sources (10) and (11).

[2] Census summary file information. Data sources (16) and (17) for 1982 and 1988, respectively (the latter is 1992 population census data).

[3] Test system information, data sources (1) and (2) for 1982 and 1988, respectively.

[4] Administrative information. Data sources (8) and (9) for 1982 and 1988, respectively.

[5] 1990 and 1992 household survey information, data sources (10) and (11).

Table 4:
Sorting in urban, non-Santiago communes, 1990

Panel A	Dep. variable - within commune observations of:			
	$\frac{Avg.\ math\ score\ in\ public\ schools}{Avg.\ math\ score\ in\ all\ schools}$ [1]		$\frac{Avg.\ repetition\ in\ public\ schools}{Avg.\ repetition\ in\ all\ schools}$ [2]	
	(1)	(2)	(3)	(4)
Private enrollment rate	-0.18***	-0.19***	0.33***	0.22**
	(0.03)	(0.05)	(0.10)	(0.10)
	[-0.72]	[-0.76]	[0.20]	[0.13]
Commune controls and regional dummies[3]	No	Yes	No	Yes
N	48	48	48	48
R^2	0.512	0.634	0.079	0.404
Panel B	Dep. variable - within commune observations of:			
	$\frac{Avg.\ SES\ in\ public\ schools}{Avg.\ SES\ in\ all\ schools}$ [4]		$\frac{Avg.\ income\ in\ public\ schools}{Avg.\ income\ in\ all\ schools}$ [5]	
	(1)	(2)	(3)	(4)
Private enrollment rate	-0.22***	-0.14**	-0.21***	-0.25*
	(0.05)	(0.07)	(0.06)	(0.13)
	[-0.59]	[-0.37]	[-0.45]	[-0.53]
Commune controls and regional dummies[3]	No	Yes	No	Yes
N	48	48	48	48
R^2	0.332	0.650	0.220	0.433

*, **, ***: significant at the 10, 5, and 1 percent level, respectively.

Huber-White standard errors are in parenthesis.

Brackets contain the proportion of a standard deviation change in the dependent variable brought about by a one standard deviation increase in the private enrollment rate.

[1] The dependent variable and the private enrollment measure are based on test scores, data source (3) in table A.1.

[2] The dependent variable and private enrollment measure are based on administrative information for 1988, data source (9).

[3] Commune controls include: literacy rate, population, population squared, mean years of educational attainment, poverty rate, and average household income. These come from CASEN and census information, data sources (12) and (17), respectively.

[4] The dependent variable and private enrollment measure are based on SIMCE information, data source (3). See text for further detail on the dependent variable's construction.

[5] The dependent variable is constructed for household survey information for 1990 and 1992, data sources (10) and (11).

Table 5: Sorting over time in urban non-Santiago communes, 1982-1988

Panel A	Dep. variable: within-commune observations of: $\frac{\text{Average score in public schools}}{\text{Average score in all schools}}$							
	Math[1]				Language[1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private enrollment rate		-0.17***	-0.16***	-0.14*		-0.20***	-0.19***	-0.39***
		(0.03)	(0.04)	(0.09)		(0.03)	(0.03)	(0.10)
		[-0.60]	[-0.56]	[-0.49]		[-0.66]	[-0.63]	[-1.29]
1988 dummy	-0.02***	-0.01	-0.01	-0.00	-0.03***	-0.01	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Commune controls[2]	No	No	Yes		No	No	Yes	
Commune dummies	No	No	No	Yes	No	No	No	Yes
N	84	84	84	84	84	84	84	84
R^2	0.086	0.381	0.417	0.849	0.128	0.500	0.529	0.855
Panel B	Dep. variable: within-commune observations of: $\frac{\text{Average characteristic in public schools}}{\text{Average characteristic in all schools}}$							
	Socioeconomic status[3]				Repetition rate[4]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private enrollment rate		-0.22***	-0.14***	-0.45**		0.80***	0.51***	1.05**
		(0.05)	(0.06)	(0.20)		(0.18)	(0.19)	(0.43)
		[-0.46]	[-0.29]	[-0.94]		[0.46]	[0.29]	[0.60]
1988 dummy	-0.06***	-0.03***	-0.04***	-0.01	0.10**	0.02*	0.05	-0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.05)	(0.05)	(0.05)	(0.05)
Commune controls[2]	No	No	Yes		No	No	Yes	
Commune dummies	No	No	No	Yes	No	No	No	Yes
N	84	84	84	84	96	96	96	96
R^2	0.192	0.368	0.439	0.749	0.040	0.216	0.468	0.847

*, **, ***: significant at the 10, 5, and 1 percent level, respectively.

Huber-White standard errors are in parenthesis.

Brackets contain the proportion of a standard deviation change in the dependent variable brought about by a one standard deviation increase in private enrollment.

[1] The dependent variables are based on test system information for 1990, data sources (1) and (2) in table A.1. Private enrollment is drawn from administrative information, data sources (8) and (9).

[2] Commune controls include: literacy rate, population, population squared, mean years of educational attainment, poverty rate, and average household income. These come from CASEN and census information, data sources (12) and (17).

[3] Data sources are the same as for the regressions in Panel A. See text for further details on the construction of the dependent variable.

[4] The dependent variable and private enrollment rates are drawn from administrative information, data sources (8) and (9).

Table 6: Are private schools better? The standard approach

	Dependent variable: school-level math score[1]				
	(1)	(2)	(3)	(4)	(5)
Voucher school dummy	6.0***	1.6***	1.4***	2.0***	2.5***
	(0.4)	(0.4)	(0.4)	(0.4)	(0.4)
	[0.53]	[0.14]	[0.12]	[0.18]	[0.22]
School: average parental schooling[2]					
College graduate		21.1***	18.9***	20.1***	21.3**
		(2.9)	(2.9)	(3.2)	(3.5)
High school graduate or some college		15.8***	15.2***	14.8***	14.4***
		(1.8)	(1.8)	(1.8)	(1.7)
Primary graduate or some high school		9.2***	8.7***	8.6***	8.5***
		(1.7)	(1.7)	(1.7)	(1.6)
Some primary		1.7	1.7	1.6	1.6
		(1.7)	(1.7)	(1.7)	(1.6)
School: average expenditure[3]					
Low		1.7***	1.6***	1.3***	1.8***
		(0.4)	(0.4)	(0.4)	(0.4)
Medium		3.1**	3.5**	2.5*	3.3**
		(1.5)	(1.5)	(1.5)	(1.5)
High		3.8	4.6	5.2	3.3
		(4.4)	(4.3)	(3.6)	(2.6)
School: 4th grade enrollment		0.4***	0.4***	0.4***	0.4***
		(0.0)	(0.0)	(0.0)	(0.0)
Commune: literacy rate			0.2**	0.1	–
			(0.1)	(0.1)	
Commune: population			-2.7***	-2.1**	–
			(0.7)	(0.8)	
Commune: population squared			0.5***	0.4*	–
			(0.2)	(0.2)	
Commune: mean years of schooling			0.1	0.4	–
			(0.3)	(0.4)	
Commune: poverty rate			-0.1**	-0.1*	–
			(0.0)	(0.0)	
Commune: mean income			0.1	0.0	–
			(0.1)	(0.0)	
13 region dummies	No	No	No	Yes	No
319 commune dummies	No	No	No	No	Yes
N	3,670	3,670	3,670	3,670	3,670
R^2	0.064	0.283	0.292	0.317	0.382

*, **, ***: significant at the 10, 5, and 1 percent level, respectively.

Huber-White standard errors are in parenthesis.

Brackets contain the proportion of a standard deviation change in the test score represented by the coefficient on the voucher school dummy.

[1] The sample includes municipal and voucher schools in all communes. The dependent variable and the private enrollment rate are drawn from test system information, data source (3) in table A.1. The commune controls come from CASEN and census information, data sources (12) and (17).

[2] Excluded category: No schooling.

[3] Excluded category: Lowest spending.

Table 7:
Does competition force public schools to improve? The standard approach

	Dependent variable: school-level math score[1]			
	(1)	(2)	(3)	(4)
Commune: private enrollment rate	4.8**	-1.6	-8.2***	-7.5**
	(1.9)	(1.7)	(2.0)	(3.0)
	[0.09]	[-0.03]	[-0.15]	[-0.14]
School: average parental schooling[2]				
College graduate		20.0***	15.5***	16.3***
		(3.0)	(3.2)	(3.7)
High school graduate or some college		14.9***	13.5***	13.5***
		(2.6)	(2.7)	(2.8)
Primary graduate or some high school		7.3***	6.6***	6.6***
		(2.0)	(2.1)	(2.1)
Some primary		1.1	1.3	1.3
		(2.0)	(2.0)	(2.0)
School: average expenditure[3]				
Low		1.7**	1.4**	1.4**
		(0.7)	(0.6)	(0.6)
Medium		3.3	2.9	4.3
		(7.1)	(6.6)	(6.7)
School: 4th grade enrollment		0.4***	0.4***	0.4***
		(0.1)	(0.7)	(0.1)
Commune: literacy rate			0.2	0.1
			(0.2)	(0.2)
Commune: population			-2.2	1.2
			(1.0)	(1.2)
Commune: population squared			0.3	0.2
			(0.3)	(0.3)
Commune: mean years of schooling			0.9*	1.2*
			(0.7)	(0.7)
Commune: poverty rate			-0.1**	-0.1**
			(0.1)	(0.1)
Commune: mean income			0.0	0.1
			(0.3)	(0.3)
13 region dummies	No	No	No	Yes
N	2,395	2,395	2,395	2,395
R^2	0.0072	0.155	0.179	0.201

*, **, ***: significant at the 10, 5, and 1 percent level, respectively.

Huber-White standard errors are in parenthesis.

Brackets contain the proportion of a standard deviation change in the test score represented by the coefficient on the voucher school dummy.

[1] The sample includes only municipal schools (in all communes). The dependent variable and the private enrollment rate are drawn from test score information, data source (3) in table A.1. The commune controls come from CASEN and census information, data sources (12) and (17).

[2] Excluded category: No schooling.

[3] Excluded category: Lowest spending.

Table 8:
Private enrollment and average outcomes, 1990

Outcome:	Sample: all communes		Sample: urban non-Santiago communes	
	(1)	(2)	(3)	(4)
Panel A: Average math score in all schools[1]				
Private enrollment rate	12.1***	2.2	3.9	-3.4
	(2.0)	(2.2)	(3.6)	(3.6)
	[0.41]	[0.07]	[0.14]	[-0.12]
Commune controls and regional dummies[2]	No	Yes	No	Yes
N	180	180	48	48
R^2	0.168	0.671	0.025	0.771
Panel B: Average repetition rate in all schools[3]				
Private enrollment rate	-0.05***	0.01	-0.05*	0.02
	(0.01)	(0.01)	(0.03)	(0.02)
	[-0.29]	[0.06]	[-0.35]	[0.14]
Commune controls and regional dummies[2]	No	Yes	No	Yes
N	180	180	48	48
R^2	0.087	0.677	0.077	0.758
Panel C: Years of schooling among 10-15 yr. olds[4]				
Private enrollment rate	0.61***	0.12	0.11	0.01
	(0.17)	(0.69)	(0.18)	(0.11)
	[0.05]	[0.01]	[0.01]	[0.00]
Commune controls and regional dummies[2]	No	Yes	No	Yes
N	24,652	24,652	15,854	15,854
R^2	0.003	0.014	0.000	0.007

*, **, ***: significant at the 10, 5, and 1 percent level, respectively.

Huber-White standard errors are in parenthesis.

Brackets contain the proportion of a standard deviation change in the independent variable brought about by one standard deviation increase in the private enrollment rate.

[1] The dependent variable and the private enrollment rate are based on test score information, data source (3) in table A.1.

[2] Commune controls include: literacy rate, population, population squared, mean years of educational attainment, poverty rate, and average household income. These come from CASEN and census information, data sources (12) and (17).

[3] The dependent variable and private enrollment rate are based on administrative information for 1988, data source (9).

[4] The dependent variable is drawn from 1990 and 1992 CASEN information, data sources (10) and (11). The private enrollment rate is from SIMCE information, data source (3).

Table 9: Private enrollment and achievement over time - 1982-1988[1]

Outcomes	All communes with data				Urban, non-Santiago communes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Math scores[1]								
Private enrollment rate		0.0	-7.5***	-7.4*		6.2*	-2.3	5.4
		(3.1)	(2.7)	(5.2)		(3.5)	(3.7)	(10.0)
		[0.00]	[-0.16]	[-0.16]		[0.24]	[-0.08]	[0.20]
1988 dummy	1.8	1.8	2.5***	2.5***	0.4	-0.2	0.1	-0.1
	(0.8)	(0.9)	(0.7)	(0.6)	(0.9)	(0.9)	(0.7)	(1.1)
Commune controls[2]	No	No	Yes		No	No	Yes	
Commune dummies	No	No	No	Yes	No	No	No	Yes
N	172	172	172	172	84	84	84	84
R^2	0.027	0.027	0.424	0.860	0.003	0.040	0.438	0.833
Panel B: Repetition[3]								
Private enrollment rate		-0.04***	0.00	0.06**		-0.07**	-0.02	0.08
		(0.01)	(0.01)	(0.03)		(0.03)	(0.02)	(0.05)
		[-0.17]	[0.01]	[0.25]		[-0.27]	[-0.07]	[0.31]
1988 dummy	-0.03***	-0.03***	-0.03***	-0.03**	-0.02***	-0.02**	-0.02***	-0.03***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Commune controls[2]	No	No	Yes		No	No	Yes	
Commune dummies	No	No	No	Yes	No	No	No	Yes
N	310	310	310	310	96	96	96	96
R^2	0.152	0.179	0.453	0.842	0.106	0.172	0.585	0.883
Panel C: Mean yrs. of schooling for 10-15 yr. olds[4]								
Private enrollment rate		0.19*	-0.33**	-0.87***		-0.22	-0.19	-0.40
		(0.22)	(0.15)	(0.25)		(0.19)	(0.21)	(0.30)
		[-0.01]	[-0.02]	[-0.05]		[-0.02]	[-0.02]	[-0.04]
1990/92 dummy	0.76***	0.74***	0.81***	0.85***	0.68***	0.70***	0.74***	0.75***
	(0.05)	(0.05)	(0.04)	(0.03)	(0.05)	(0.05)	(0.06)	(0.04)
Commune controls[2]	No	No	Yes		No	No	Yes	
Commune dummies	No	No	No	Yes	No	No	No	Yes
N	68,366	68,366	68,366	68,366	14,769	14,769	14,769	14,769
R^2	0.029	0.039	0.052	0.060	0.028	0.024	0.031	0.037

*, **, *** significant at the 10, 5, and 1 percent level, respectively. Huber-White standard errors are in parenthesis.

Brackets contain the proportion of a std. dev. change in the indep. variable brought about by one std. dev. increase in private enrollment.

[1] The dependent variable is based on test system information, data sources (1) and (2) in table A.1.

[2] Commune controls include: literacy rate, population, population squared, mean years of educational attainment, poverty rate, and average household income. These come from CASEN and census information, data sources (12) and (17).

[3] The dependent variable is based on administrative information, sources (8) and (9).

[4] The dependent variable and the private enroll. rate are based on 1982 census and 1990/92 CASEN information, data sources (15), (10), and (11).

Table 10:
Private enrollment and educational attainment 1982-1990/92

	Dependent variable: Mean years of schooling among 10-15 year olds[1]					
	All communes			Urban, non-Santiago communes		
	(1)	(2)	(3)	(4)	(5)	(6)
Group 1[2]	-1.01***	-1.06***	-0.89***	-0.84***	-0.85***	-0.80***
	(0.04)	(0.04)	(0.03)	(0.04)	(0.05)	(0.03)
Group 2[2]	-0.33***	-0.39***	-0.34***	-0.30***	-0.34***	-0.32***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)
Group 4[2]	0.36	0.38	0.37	0.37	0.39	0.38
	(0.03)	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)
Group 1*90/92 dummy		0.84***	0.94***		0.74***	0.86***
		(0.05)	(0.05)		(0.07)	(0.08)
Group 2*90/92 dummy		0.58***	0.69***		0.63***	0.62***
		(0.05)	(0.05)		(0.05)	(0.08)
Group 3*90/92 dummy		0.39***	0.50***		0.44***	0.48***
		(0.04)	(0.05)		(0.05)	(0.06)
Group 4*90/92 dummy		0.40***	0.46***		0.40***	0.47***
		(0.06)	(0.08)		(0.07)	(0.10)
Group 1*90/92 dummy*Δ priv. enroll.			-1.03**			-0.97*
			(0.42)			(0.56)
Group 2*90/92 dummy*Δ priv. enroll.			-0.23			0.47
			(0.43)			(0.62)
Group 3*90/92 dummy*Δ priv. enroll.			-0.32			-0.11
			(0.34)			(0.40)
Group 4*90/92 dummy*Δ priv. enroll.			-0.05			-0.03
			(0.58)			(0.70)
Commune dummies	No	No	Yes	No	No	Yes
N	63,271	63,271	63,271	40,418	40,418	40,418
R^2	0.054	0.072	0.087	0.040	0.056	0.063

*, **, *** - significant at the 10, 5, and 1 percent level, respectively.

[1] The dependent variable is based on 1982 Census and 1990/92 CASEN household survey information, data sources (45), (10) and (11) in table A.1. The private enrollment rate comes from administrative information, data sources (8) and (9).

[2] Group 1: household heads with less than 5 years of schooling.
Group 2: household heads with between 6 and 8 years of schooling.
Group 3: household heads with between 9 and 12 years of schooling (excluded group).
Group 4: household heads with more than 13 years of schooling.

Table 11:

1982-88 commune level changes in private enrollment, and pre-existing and concurrent trends

Independent variable:	Dependent variable: 1982-1988 change in private enrollment:			
	(1)	(2)	(3)	(4)
Panel A: Relative public performance in 1982				
$\frac{\text{Average score in "middle class" public schools}}{\text{Average score in "middle class" private schools}}$[1]	-0.04			
	(0.07)			
$\frac{\text{Prop. of "middle class" 13 year-olds with primary completed}}{\text{Prop. of all 13 year-olds with primary completed}}$[2]		-0.03***		
		(0.01)		
R^2	0.027	0.026		
N	103	88		
Panel B: Pre-existing trends				
1980-82 change in private enrollment[3]	-0.004			
	(0.025)			
1978-1982 change in proportion of schools private[4]		-0.005		
		(0.022)		
1970-1982 change in avg. yrs. of schooling, 10-15 year-olds[5]			0.023**	
			(0.006)	
1970-1982 change in avg. yrs. of schooling, among[5] "middle class" 10-15 year-olds				0.014**
				(0.006)
R^2	0.000	0.000	0.073	0.023
N	259	295	224	224
Panel C: Concurrent trends				
1982-1990 change in the % of families who own their home[6]	0.124**			
	(0.056)			
1982-1990 change in the % of families with phones[6]		0.246***		
		(0.042)		
R^2	0.047	0.260		
N	103	103		

*, **, *** - significant at the 10, 5, and 1 percent level, respectively.

[1] Data source (1) in table A.1. "Middle class" refers to the middle of three SES categories the Ministry of Education used to classify schools in 1982

[2] Data source (14). "Middle class" refers to children from households where the household head has at least a primary education, but is not a high school or college graduate.

[3] Data sources (7) and (8).

[4] Data sources (18) and (8).

[5] Data sources (14) and (15).

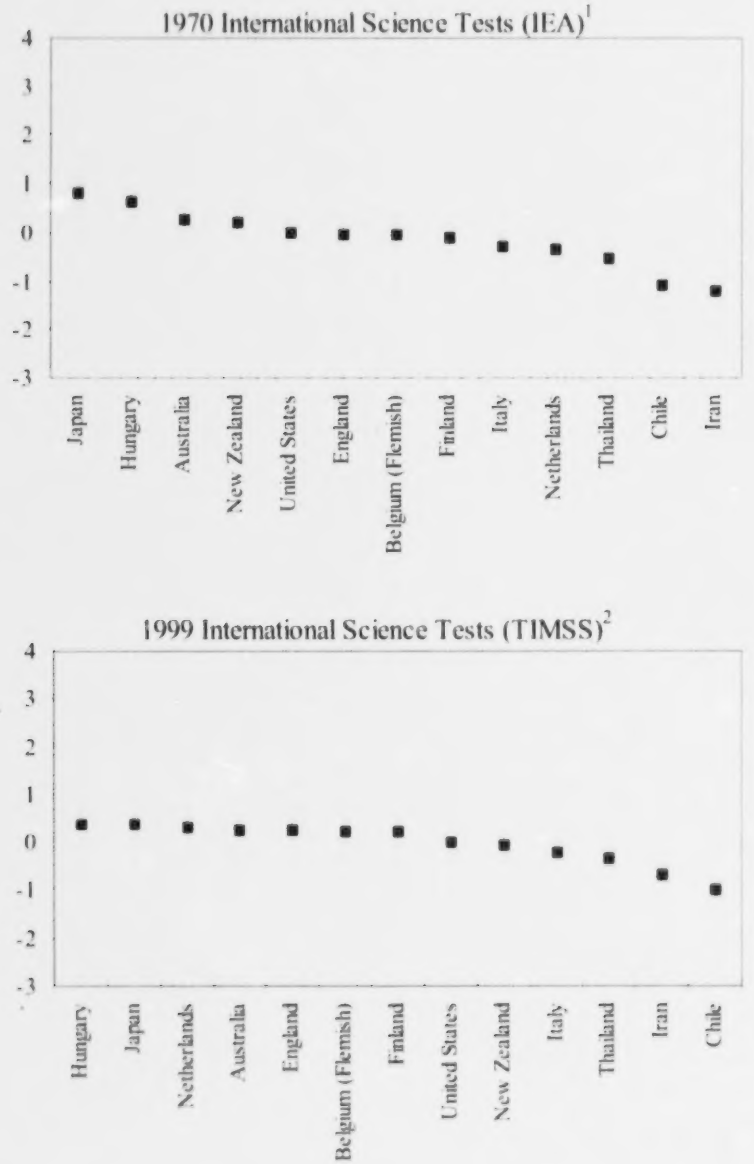
[6] Data sources (15) and (10). Refers only to households with children.

Table A.1
Data sources

Data type and source	Original unit of observation	Year									
		1970	1978	1980	1982	1988	1990	1992	1996	1998	1999
Test scores											
PER[1]	School				(1)						
SIMCE[2]	School					(2)	(3)		(4)		
TIMSS[3]	Country										
International science exams[4]	Country	(6)									(5)
Administrative data											
Enrollment files	School			(7)	(8)	(9)					
Surveys											
CASEN[5] household survey	Individual						(10)	(11)	(12)	(13)	
Census data											
Micro data	Individual	(14)			(15)						
Summary files	Commune				(16)			(17)			
Other											
School maps[6]	Commune		(18)								

[1] Programa de Evaluación del Rendimiento Escolar.
[2] Sistema de Evaluación de Calidad de la Educación.
[3] Third International Mathematics and Science Study, see http://timss.bc.edu
[4] International Science Exams, International Education Association, see Comber and Keeves (1973).
[5] Encuesta de Caracterización Socioeconómica Nacional.
[6] See Instituto Geográfico Militar (1983).

Figure 1: Chile's ranking in international tests, 1970 and 1999



[1] International Science Exams, International Education Association, data source (6) in table A.1.
[2] Third International Math and Science Study, data source (5).
Note: All scores are standardized by subtracting the U.S. mean and dividing by the U.S. standard deviation. The square represents each country's median score, and the endpoints are found adding and subtracting two standard deviations.

Figure 2: National enrollment shares by sector, 1970-1995[1]

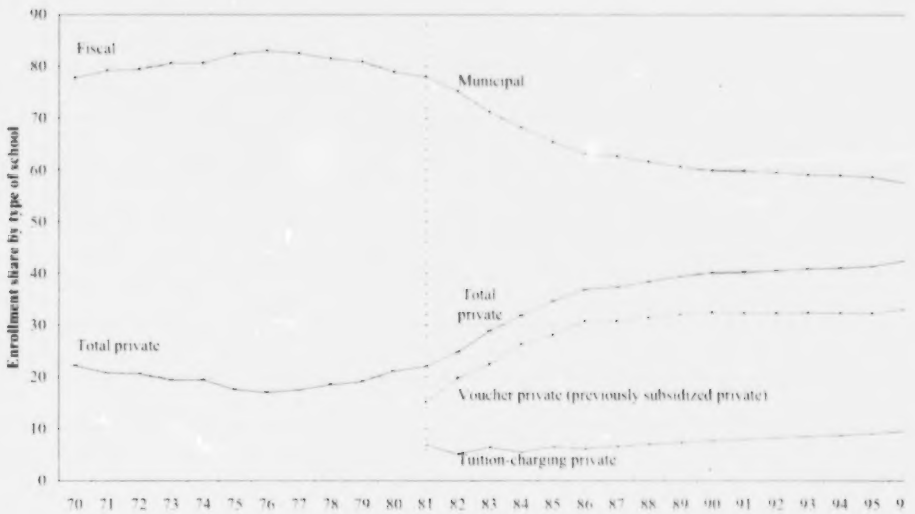
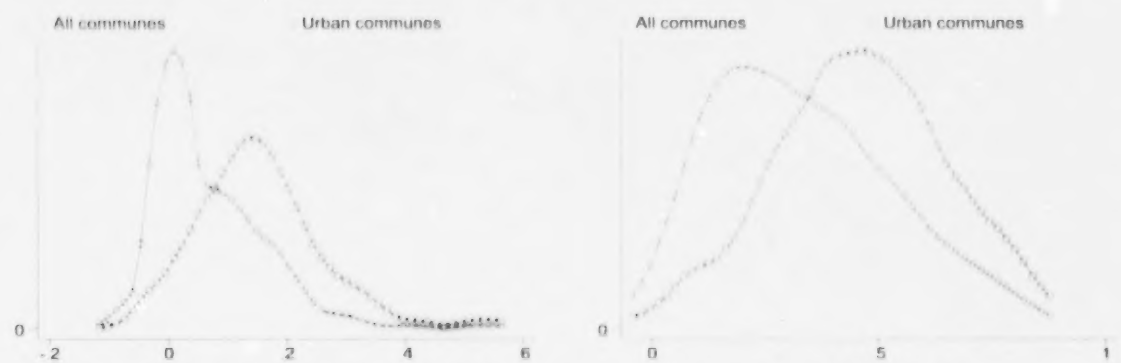


Figure 3: Private enrollment among communes[2]



Panel A: Within-commune changes, 1982-96

Panel B: Levels, 1996

[1] Data assembled from several issues of the Ministry of Education's *Compendio Estadistico*.

[2] -- Panel A is based on administrative information, data sources (8) and (9) in table A.1. It covers all communes in Chile.

-- Panel B is based on test system information -- data source (4) -- and covers communes with positive private enrollment.

Figure 4: Number of schools by sector, 1980-1995[1]

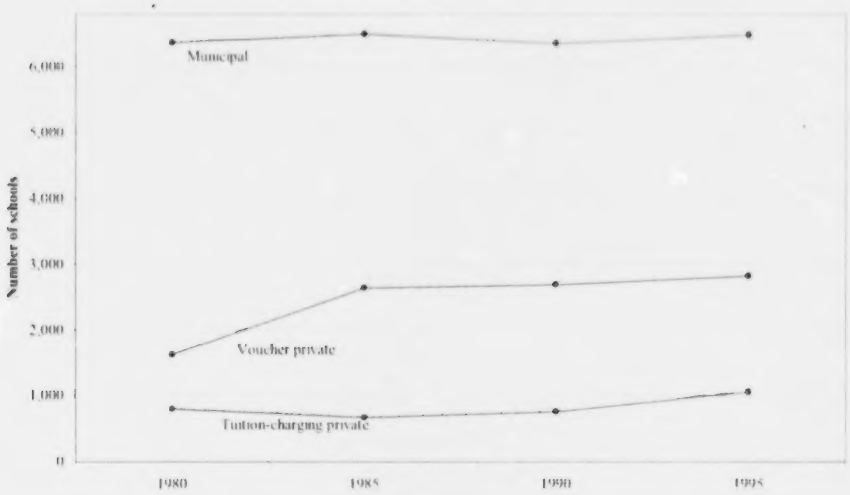
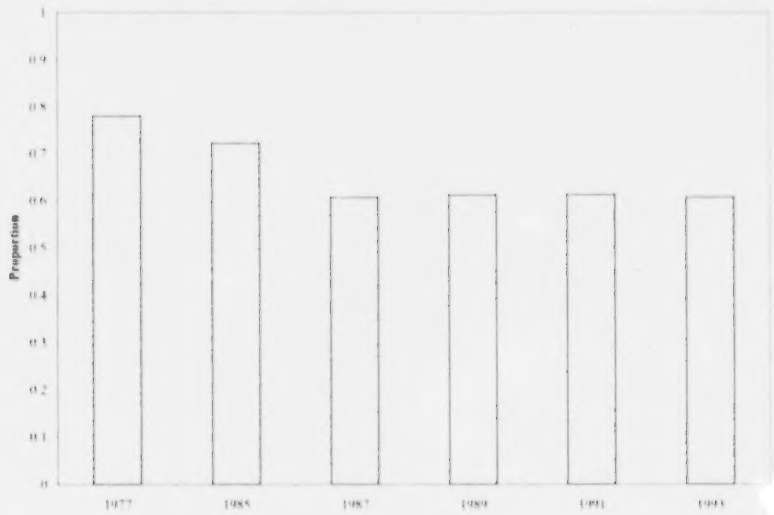


Figure 5: Proportion of teachers in the public sector, 1977-1993[1]



[1] Data assembled from several issues of the Ministry of Education's *Compendio Estadistico*.

Figure 6: Public performance and private enrollment among communes, 1990

-- Based on test system information, data source (3) in table A.1.
-- For each commune, the variable on the y-axis is the ratio of the average math score in public schools, and the average math score in all schools.
-- The size of each observation is proportional to the commune's population.

Figure 7: Distribution of incumbent and entrant voucher schools by SES, 1988[1]

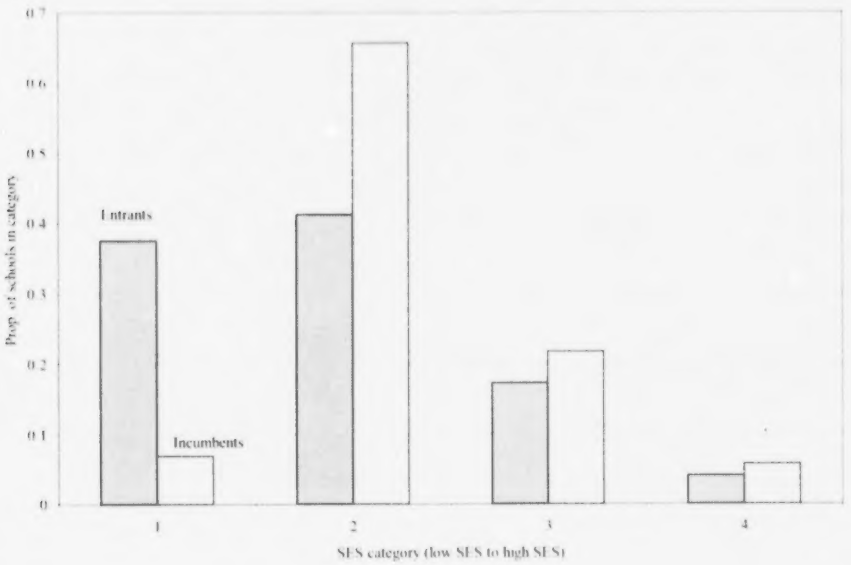
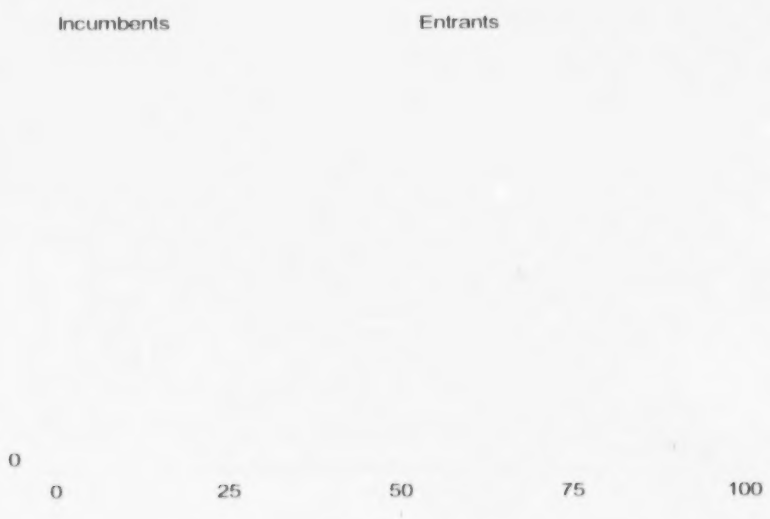
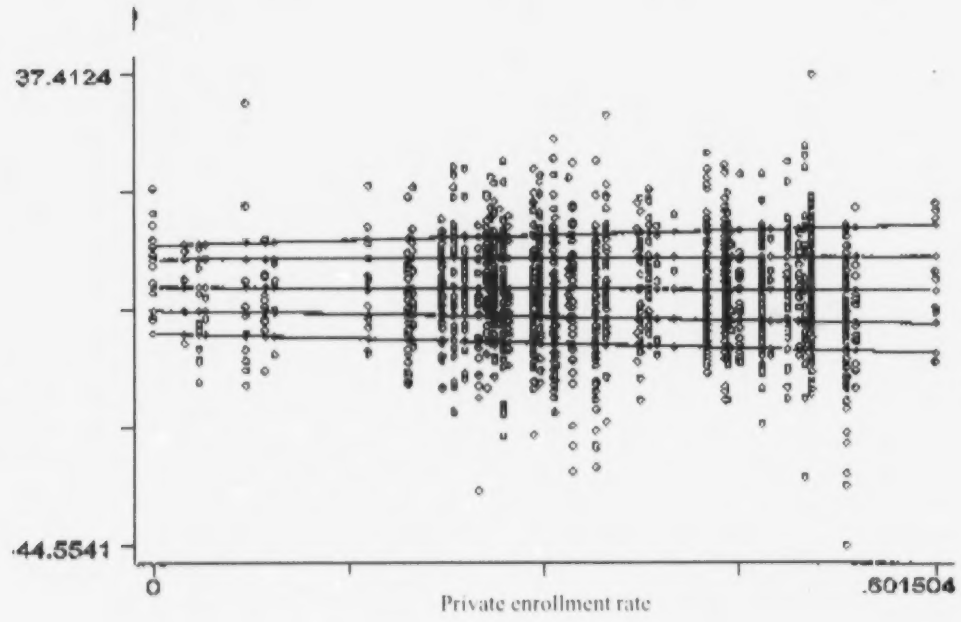


Figure 8: Distribution of math scores for voucher schools, by incumbent status, 1988



[1] Both figures refer only to uban communes (as defined in the text) and are based on test system information, data source (2) in table A.1. Incumbent/entrant status is determined using sources (1) and (2).

Figure 9: Private enrollment and the distribution of test scores, 1990



Note: Observations are residuals of regressions of math test scores on a school's characteristics.

Figure 10: Chile's ranking in international tests, 1970 and 1999[1]

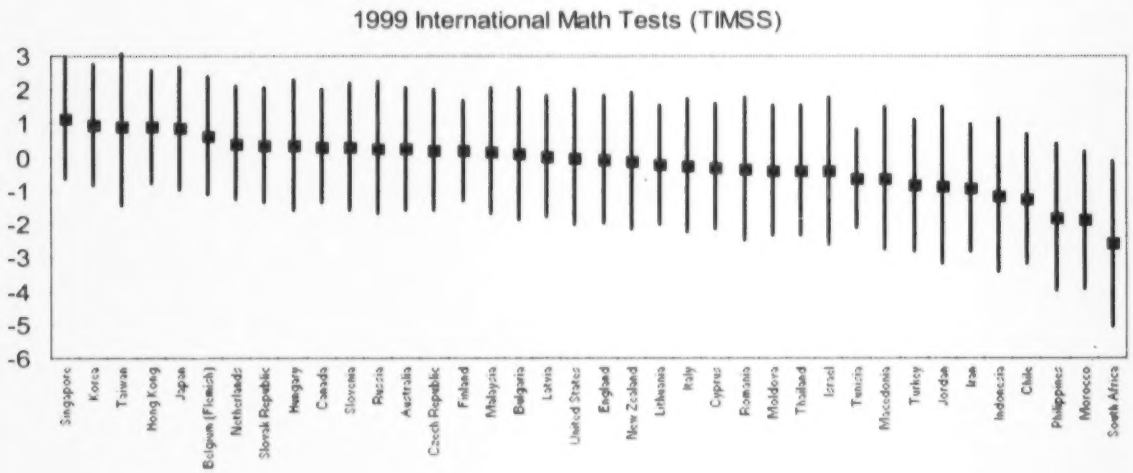
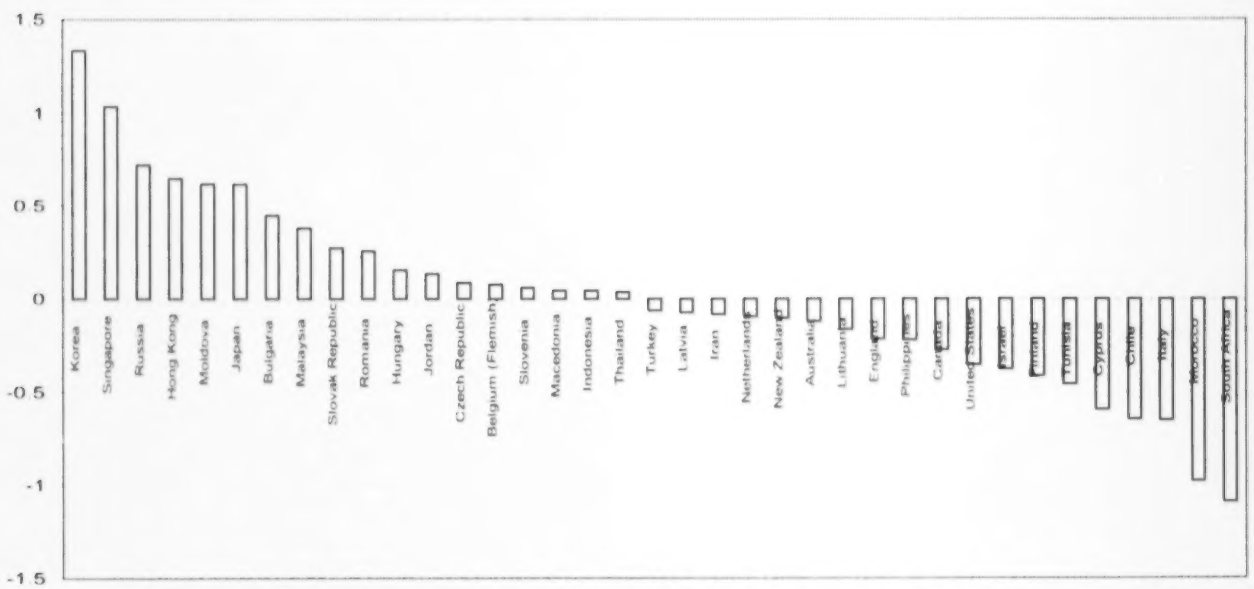


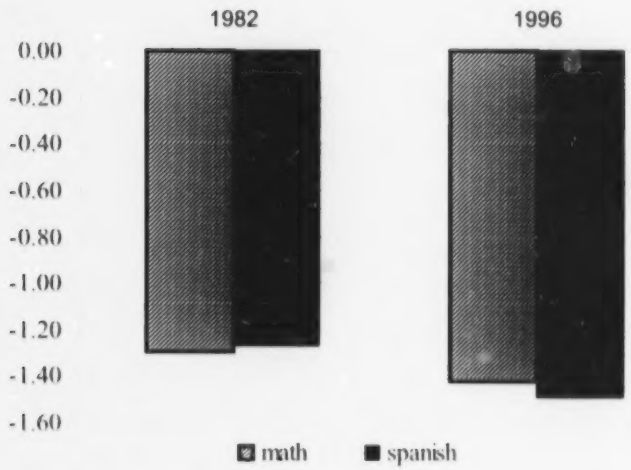
Figure 11: Residuals from a regression of the median country score on log GDP per capita, the pupil-teacher ratio, and the net enrollment rate[2]



[1] Third International Math and Science Study, data source (5) in table A.1.

[2] The dependent variable is from the TIMSS, data source (5). The independent variables, which refer to the primary level, are from the *World Education Report*, 2000 (UNESCO); and the *World Development Report*, 2000 (The World Bank).

Figure 12: Average test score among municipal and voucher schools, relative to tuition-charging private schools, 1982 and 1996[1]
(Std. Deviations below tuition charging)



[1] Figure based on test score information, data sources (1) and (4) in table A.1.